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ORIGINAL COMMUNICATIONS.

History of three cases of Intermittent Fever, showing the natural course of the disease. By J. Forsyth Meigs, M. D.

The natural history of disease, or, in other words, the characters of disease uninfluenced by human interference, must always be a subject of the greatest interest to all but the merely routine practitioner. And yet the treatment of most important diseases is, at this time, so well established by general consent among physicians, that there are few opportunities of witnessing the successive phenomena of the morbid state excepting as more or less disturbed or controlled by therapeutical intervention. Especially is this true of the periodical fevers, the treatment of which is so clearly and explicitly formulated by the best authorities in medicine, that it is altogether doubtful whether a regular practitioner has any right to leave a case to the powers of nature alone. The following cases of intermittent fever occurred to me after I had been several years in practice, and were the first in which I saw the unchecked tendencies of the disease,—tendencies which showed me how comparatively impotent must have been the medical art in a contest with such a malady before the discovery of cinchona. It may be said by some that even these cases cannot be properly considered as having been left to nature's own management, since they were treated at first by the homœpathic method. I have not, however, the slightest hesitation in so considering them, believing that the influence of the

infinitesimal dosing to which they had been subjected, was, as to any real effect, equivalent to nothing. They interested me very much at the time they occurred, as proving to demonstration the manifest and most beneficent control exercised by a remedy over a disease, an effect which I had not doubted to be sure before, but which, nevertheless, was now exhibited to me in the most delightful manner.

The cases are as follows: Two sisters, one 16 years of age, the other 25, spent a part of August, 1851, at Gloucester, New Jersey. They came home in that month, and remained well through the following fall and winter. On the 5th of May, 1852, the elder sister was taken sick with tertian ague which continued during two weeks. At the end of the second week the disease assumed the quotidian type, and so continued, without the slightest break, up to the 16th of June, just six weeks from the onset. The chills occurred sometimes at midday, but generally, indeed almost always, between 3½ and 4 P. M. They lasted one or two hours usually, were violent and attended with shaking and with chattering teeth. Then came a "raging" fever, and after a few hours, a copious, wringing sweat. The patient slept very badly at night, had violent headaches, was without appetite, and vomited nearly every afternoon when the chill came on. She mentioned one curious fact, to wit: that when she lay on her right side, she rarely vomited, but instantly on turning to the left side, would vomit violently. She usually rejected large quantities of green bile.

During the first six weeks of her illness she had been frequently visited by a homœopath, who had dosed her with various infinitesimals. When I saw her on the 16th of June she was as white as chalk; her lips were white, and the mucous membrane of the mouth pale; her hands and ears were transparent; (she was naturally a healthy and hearty subject both in appearance and in fact;) the impulse of the heart was strong, jerking, and sudden, the cardiac sounds were metallic in tone and attended with a soft bruit; she was so weak that she could not stand up without giddiness, sickness, and disposition to faint. She ate almost nothing, and was as much broken down in spirits as in strength, crying constantly, as she told me, in

her bed, and entreating her mother to give up the infinitesimals and try some more potent means of cure.

My first visit was at 8 P. M., the patient having then the fever which followed her daily chill. It was too late to give quinine that evening, but on the following morning she began it, and continued to take it through that day. She had her chill and fever that day in spite of the remedy; but the following day, after taking quinine in the morning, she it missed entirely. She was now put upon the use of milk punch, small doses of quinine, iron, porter, meat, and general good diet, and rapidly recovered her usual health.

The other sister was not attended by myself, for, a few days before I was called in, after having suffered from the disease during five weeks, though less severely than my patient, she had obtained from an apothecary store some one of the usual prescriptions for intermittent fever, and was cured by it in a very short time.

The third case was one that occurred to me some years since. The subject was a boy ten years old, who had been spending the latter part of the summer on the Chesapeake Bay. Soon after his return to this city, he was attacked with intermittent fever. His mother sent for a homœopathic physician, who visited him daily and prescribed. During the first six days, the attack was of the tertian form. It then changed to a quotidian, and so continued for six days longer. On the eleventh day there appeared some hæmorrhage from the bowels, which, with other alarming symptoms, and the continuation of the paroxysms, so terrified the mother, that, on the morning of the thirteenth day, she dismissed the homœopath and sent for me. The child was much prostrated and looked ill. He had already become very thin, and was entirely without appetite. The abdomen was largely distended from two causes,—the presence of gas in the intestines, and enlargement of the spleen. The latter organ was so much increased in size as to extend nearly to the umbilicus. There was no cough nor any other sign of trouble in the thoracic organs, nor were there any important cerebral symptoms. The daily paroxysms of the disease were severe, lasting from about $10\frac{1}{2}$ or 11 A. M. to the latter part of the afternoon.

The treatment directed was small doses of sugar of lead and

opium through the day, laudanum injections at night, and a diet of farinaceous articles and light broths for the intestinal hæmorrhage; whilst quinine was given daily after the cessation of the paroxysm and prior to the attack in the morning. The quinine was given after the paroxysm which occurred on the day that I first saw the patient, and on the following day. Notwithstanding this, a paroxysm occurred on the day after my first visit (14th), but none afterwards. The remedy broke up the paroxysmal character of the disease from that moment, and the hæmorrhage ceased on the 14th day. The spleen returned rapidly to its normal size, and, with the exception of some troublesome constipation and a prostration which was slow in passing away, the patient had no further difficulty.

These cases cannot but be, it seems to me, interesting to the physician. Though they demonstrate only what we already know, the efficacy of quinine in the treatment of malarial disease, they do it in so plain and conspicuous a manner as to give to this truth a freshness which is delightful in these days of isms and quackeries, when doubt and skepticism as to the utility of the medical art are only too common. The history of the first case is particularly interesting in showing, not only the efficacy of the remedy, but also the effects of the disease, when left to its natural unchecked development, upon the constitution. We may, in reflecting upon the wretched condition of exhaustion to which the patient had been reduced, in considering the suffering she each day went through with,—the chill, the vomiting, the fever, with its aches and pains, the sweat, with its wretched prostration of soul as well as body, the misery of daily anticipation, and the sure progession of the daily weakness, learn to value the greatness of the boon conferred upon us by Providence in the discovery of the Peruvian bark.

Habit and routine familiarize us to everything, and physicians themselves are, perhaps, less thankful than they should be for the possession of a means of treatment the value of which is priceless; while the ungrateful people, who often cry out against quinine as a dangerous remedy, should learn to estimate more accurately its true preciousness.

A Case of Rheumatic Chorea.

By Henry Hartshorne, M. D.

Allusion having been made, in the interesting article of Dr. Gibbs upon Chorea, in the September number of the Examiner, to the *rheumatic* affinities of that disease, I present the following case, as affording, in a more than usually clear manner, an illustration of the occasional connection of the two disorders.

Ann Cook, aged 13, and previously healthy, was, after some household exposure to wet and cold, attacked, about the 8th of February, 1856, with acute articular rheumatism. It affected chiefly the ankles, which were much swollen, hot and painful. I was not called for several days after the commencement of the attack; but found her, then, suffering very much with the inflamed joints, and with considerable fever. A saline purge was advised, after which she took the vin. colchic. rad. in ordinary doses, with magnesia. No application was made to the feet, except laudanum. On the 14th, the articular affection entirely, and almost suddenly, subsided; with an increased violence of the pulse; and especially of the impulse of the heart, a flushed face, orthopnœa, and distressing pain in the cardiac region. A bellows murmur was heard, with the second sound of the heart: whether this had been present at any previous time, no examination had determined. Four or five ounces of blood were taken by cups from between the shoulders, followed by a blister over the sternum; and tinc. digital. was added to the colchicum mixture, which she continued to take. The violence of her symptoms was thus materially relieved; and, by the 22nd, I pronounced her well of the carditic attack, without any return of the articular rheumatism. A very slight bellows murmur only remained, with no functional derangement of the heart.

On the 1st of March, I was sent for, to treat her for *chorea*, which had gradually commenced almost from the time of cessation of the cardiac symptoms above alluded to, and had now assumed a violent form. The irregularity and spasmodic frequency of movement affected the muscles of the whole body, including the tongue, face and neck,—so that she could not speak intelligibly; nor could she walk across the room with-

out help. All of these movements subsided (as usual), entirely, during sleep.

She was, at this time, pale, rather thin, feeble, and without appetite. The treatment pursued was the usual routine of cimicifuga and mineral tonics, commencing the latter series with iron, and ending with sulphate of zinc. Salt baths, frequent dry rubbing, and friction on the spine with croton oil, were also employed.

The improvement, however, was very gradual, not approaching convalescence until about the close of April. She was then made worse by a severe fright; but continued, nevertheless, after a short interval, to improve, and was well by the early part of June.

The only particular interest in this case was the close and apparently, metastatic, succession of the three forms of disease, viz: the articular rheumatism, rheumatic carditis, and chorea. Each of these gave way just before another appeared. Except in the more strongly marked character of the cardiac inflammation, it resembled somewhat Case LXXII related by Dr. Todd (Nervous System, Phila. Ed., p. 281). While it illustrates, as we have remarked, the statements made by many clinical observers, as to the frequent association of chorea with the rheumatic diathesis, it does not at all tend to confirm the hypothesis of Simon (quoted in the paper of Dr. Gibbs referred to, Exam., Sept. 1856) in regard to the order in which the "irritation" of chorea and the "explosion" of rheumatism, are likely to occur.

Perhaps the best hypothetical mode of stating the view which the case I have narrated suggests, is—that the mobile cause (whatever its nature) to the effects of which we give the name of rheumatism, when shown in the articular, cardiac, or febrile symptoms, had, upon leaving its seat in the fibrous tissues of the heart, attacked those of the nerve-centres, sensorial or supraspinal; in the functional disturbance of which the choreic disorder consisted. It would seem thus to be analogous to the cases of *rheumatic paralysis*, so often observed.

It may not be out of place to remind the reader, that in the observations of Drs. Hughes and Burton Brown (Guy's Hospital Reports, 1846 and 1855), upon 309 cases of chorea in Guy's Hospital, they referred to rheumatism as the actual *exciting*

cause, in 13 cases; while in 58 cases where inquiry was made in regard to it, 30 had suffered from rheumatic attacks; and of 104, only 15 were free both from cardiac murmurs and from the influence of previous attacks of rheumatism.

These facts show the importance of the rheumatic diathesis in predisposing to chorea; while they do not of course controvert the generally received opinion, that disorders of nutrition, and morbid impressions upon the nervous system, account for it in a still greater number of cases.

A singular Case of Death subsequent to delivery. By O. C. Gibbs, M. D., of Frewsburg, Chautauque Co., N. Y.

In the Medical Examiner for August, Dr. Gemmill communicates a "singular case of death, occurring twenty-two days after instrumental delivery," which brings to my mind a case presenting several points of resemblance, which came under my observation nearly two years since. As the case presents some points of interest I forward you a brief report of it.

November 1st, 1854, I was called to see Mrs. A., and, on arriving at her residence, found her in labor at full term. Mrs. A. was about 35 years of age, and had given birth to a living child at full term some twelve years previously, since which time she had had four premature deliveries at seven months. Labor progressed favorably and she gave birth to a healthy female child, about six hours after I was called to the case.

After her first confinement she had suffered very much from *phlegmasia dolens*, in consequence of which she was fearful that some untoward symptoms would interfere with speedy recovery in the present instance. I saw her occasionally for the first ten days, but as no symptoms requiring treatment presented themselves I did not further enquire after the case.

November 20th, about three weeks from the time of confinement, I was again summoned to see her, about ten o'clock in the evening. The husband, who came for me, said that he did not think there was any necessity for alarm, as probably the symptoms were wholly hysterical. On arriving at the bedside of my patient, I found her laboring under the greatest dyspnœa. The number of respirations per minute was treble that of health, and

the countenance was expressive of very great distress. Air seemed to enter the lungs freely, but seemed utterly unavailing for all the purposes for which respiration is accomplished. Any attempt to examine the lungs or heart, seemed to aggravate her distress, and, in the tumultuous manner in which these organs were performing their functions, such examination would doubtless have led to no satisfactory result. The pulse was so rapid that it could not be well counted. From the husband, I learned that she had been apparently doing well, until about an hour before I saw her. She had been sitting up every day for a week or more, and had taken the principal charge of her child. She had suffered somewhat from pain in her legs, but as they were not swollen much, if any, he had not sought my advice in regard to that symptom, and, in fact, for the last few days she had even improved in that particular. She had complained that her lochial discharges were more offensive than usual, but, on the whole, considered herself to be making a fortunate recovery. On the evening of the attack, she had been up until about nine o'clock, had prepared her child and herself for bed, laid down and had gotten nearly to sleep, when the paroxysm of dyspnœa came on.

She lingered about thirty hours, without any abatement of the symptoms, and unaffected by treatment, being perfectly concious until within a few minutes of the last respiratory effort. In this case, as in that reported by Dr. Gemmill, no post mortem examination was permitted, much to my regret. The patient had never complained of heart disease, and, though intimately acquainted with her for years, my attention had never been directed to that organ as subject to abnormal action. I diagnosticated fibrinous concretions or coagulative accumulation within the heart. It is quite possible, I think, that a fibrinous deposit, breaking loose from one of the valves of the heart, had caught in the trunk of the pulmonary artery, thus accounting for some of the peculiar respiratory symptoms. If any of the readers of the Examiner, more fortunate than Dr. Gemmill and myself, have seen the autopsy of a case like the above and under similar circumstances, I trust they will report the result of their examination.

Prolonged Vitality of Small-pox Contagion. By S. E. McKinley.

To the Editor of the Examiner :—

Dear Sir,—It is sometimes instructive, and at all times interesting to the *literati* in medicine, to read of remarkable eccentricities of things pertaining to medical science, whether they have a direct bearing upon *practical medicine*, the great *ultimatum* of the science, or upon the intricate and diversified mazes of its theory. Whatever is commonplace, however momentous, is beheld with indifference, and scarcely merits, in the sovereign opinion of the multitude, a passing remark. On the contrary, whatever assumes the character of novelty, anything making a departure from the ordinary course of operations, is beheld with astonishment.

The terrors of yellow fever in one section of our country, are scarcely felt by its "fixed inhabitants;" yet yellow fever is none the less destructive because it is a household word there; nor is its cause any better known by reason of its frequent visitation; yet every person, almost, pretends to cure it. In some measure the same remark is applicable to cholera; but more especially is it so to that loathsome disease named after the presumptuous shepherd of king Alcithons—Syphilis. This demon has its centre everywhere, nor do the extremest bounds mark with any certainty the limits of a remotely probable circumference. It is ubiquitous; its habitat is everywhere; it descends from the lofty mansion to the dweller in the lowly cot, and in return radiates from the purlieus of vice, midst ribald congregations of abandoned wretches, to the polished inmate of the lordly palace. With all our familiarity with these "very popular" diseases, what do we know of them? We have been taught their pathology, arrest and cure; their eccentricities, peculiarities, and caprices we know; we can go no farther. Reason has its legitimate province; a doctrine may be above and beyond, but not opposed to our reason.

Permit me to offer a familiar illustration of our defective, if not our positive want of knowledge of the "*virus variolæ habitudo.*" It goes to establish, beyond the remotest doubt, that

atmosphere impregnated with the ethero-volatile principle of small-pox, may be confined in and hover about certain other things, and finally, after the lapse of years, induce the very disease from which it first arose.

In the year 1851, small-pox prevailed somewhat extensively in Oglethorpe Co., in this State; it was not confined to villages alone, but introduced itself upon the more rural inhabitants of the inner country, and disturbed the otherwise prevailing quiet. During this year, a family attacked by it, had, after their recovery, their house cleansed and white-washed, and all clothes worn by the sick either burned or destroyed. Notwithstanding this, it re-appeared in the same family last July, 1856.

Three years after *the first* visitation, the wife of the husband died, and quite recently he married again. The new wife, upon being installed empress of the household *de facto et de jure*, was inspired with similar sentiments to those of the redoubtable Crockett when first elected to Congress, viz.: to "bring about a radical change in the judiciary." True to her ideas of reform, and to bring the household under the auspices of a different system of diplomacy, she instituted and brought into immediate action a host of legislative operations, which led to the most astounding developments. Bed clothes, coats, trowsers, (old and new,) vests, shirts, drawers, stockings, hats, bonnets, shawls, dresses, etc., carpets, desks, bureaus, cupboards, and various other articles were disturbed in their quiet security, and exposed to the solar elements and soap and water. Very shortly after this tearing up of things, this universal exhumation, and when just about to enjoy the full fruition of the reform system, this amiable and good woman was taken with confluent small-pox, together with a servant, who did not have it in 1851. Neither wife or servant had been from home, nor had any suspicious person been to see them. They had not been to or passed through any town. They lived five miles out in the country from Lexington, and had no intercourse whatever with any person from whom they could have taken it. The writer has been requested to notice this in some journal, and if it be of any importance it is at your service.

Crook's Cross Roads, Hancock Co., Ga.

Treatment of Neuralgia by the Valerianate of Ammonia.

(Translated for the Examiner.)

Dr. Declat relates, in the Bulletin de Therapeutique, several cases which prove the happy effects that may be anticipated from the use of the above remedy.

Case 1st. Madame the Marchioness of Fontanelle, (the lady has authorised us to give her name,) was attacked six years ago with facial neuralgia of the most severe description. The pain was first experienced while cutting a wisdom tooth, which was late in making its appearance. As this tooth came through in a wrong direction, Drs. A. Legrand and Jobert de Lamballe ordered its extraction. The pain was so violent that Madame de Fontanelle was unable to open her mouth, and they were obliged to give her chloroform.

In presence of the consulting physicians, Mr. Evans performed the operation of extraction with great skill. After the removal of the tooth, the neuralgia still continued. All the ordinary means were employed to relieve it; internally sulphate of quinine, opium, belladona, sulphate of strychnia, iron, gold, and quin-quina were administered, as well as external applications of opium plaster, blisters, morphia, dulcamara, chloroform, collodion, aconite, &c. &c.

Professors Sedillot and Velpeau saw the patient without being able to give her any relief. Professor Jobert de Lamballe proposed and obtained permission to apply the actual cautery over the course of the inferior maxillary nerve. This treatment, so terrifying to a woman, slightly lessened the acuteness of the pain, but failed to overcome it; and though suffering less, Madame de Fontanelle could neither eat nor sleep; being compelled, during at least six months, to have recourse to nutritive enemata, and tonic baths to preserve her health and life.

The waters of Plombiéres diminished, for a time, the frequency of the pains; during the second season, no benefit was derived from their action; the third, her malady was increased. She was in this suffering state when, on the 19th December, 1855, she was placed under my care.

The amelioration of her complaint produced by the waters of

Plombiéres during the first season, determined me to use Fowler's solution.

The invalid consented the more willingly to this means as she preferred death, she said, to insanity from suffering. One of her friends, also, Madame de Balzac, had written to her from Germany, that this remedy was in frequent use in the country in which she lived, and that it had, to her knowledge, cured more than one case of neuralgia.

From the 19th to the 22d of December, 12 drops in the morning, 12 drops at noon, and 12 drops in the evening of the following mixture were given her:

Fowler's solution, - - - - -	¼
Mint water, - - - - -	¾

On the 22d, there was a little improvement, but the tongue was red, and she suffered much pain in the stomach; Madame de Fontanelle would not consent to diminish the next dose, as the slight amelioration she had experienced made her sanguine of more complete relief.

On the 24th, vomiting, diarrhœa, cramps in the stomach and pains returned. We discontinued the medicine.

On the 3d of January, 1856, the agony was unendurable, and my patient was in despair. I tried the valerianate of ammonia.

A tea-spoonful, taken in the evening, modified the pain at night, and rendered it bearable. Two tea-spoonsful the next day gave complete relief.

On the 6th of January, the patient could rise and speak.

On the 19th, she half opened her mouth and began to eat. The 3d of February, Madame de Fontanelle came up to me smilingly, and welcomed me, saying, "Dr. I have been well enough to dine in town; I can laugh; in society they look upon me as one risen from the dead." We gradually increased the dose to a dessert spoonful morning and evening; the improvement now became so great that her countenance resumed an entirely different aspect, and her appetite came back as hope returned.

Finally, on the 6th of May, the pains having for several days entirely ceased, we suspended the use of the medicine. Several weeks passed without a single twinge; but at the return of the

first pain, the Marchioness snatched the bottle and took a dose of the valerianate.

From time to time, some shooting pains were felt; but every time the valerianate was resorted to they disappeared. The improvement continues, and there is nothing to cause us to anticipate that the remedy will lose its efficacy should the disease return.

The case given above is one of importance. From the first the patient had been attended with care, and even affection, by the most eminent physicians; for six years almost every known means had been employed, without results of any duration.

This case of neuralgia was much more obstinate and unmanageable from its being an hereditary affection. Madame de Fontanelle's mother had suffered fearfully from a similar neuralgia. Her brother, the Count of Essex, (sic!) has had tic douloureux from his youth; and he is as well known in England from the great suffering he has gone through from it as from his high position.

Doctor A. Legrand has, throughout, watched this cure which he had pronounced hopeless six years ago; wishing himself to verify the value of the new medicine, he ordered it in the same doses to Madame de V——, whom he considered equally incurable. We know that the relief has been quite prompt; but we understand, from his having too rapidly increased the dose, that some cerebral disturbance was produced. These symptoms, however, disappeared as soon as the valerianate was given in proper doses. At present, Madame de V—— considers herself cured.

Case II. M. E. Letellier accompanied his wife to Plombières. During his sojourn at the waters, in the beginning of October, he was attacked with a pain in the head; this pain extended to the muscles of the neck, passed through the top of the head and lost itself in the branches of the facial nerve. It was impossible for the patient to raise his head from the pillow. Various remedies were tried at Plombiéres, but the pains increased and the sufferer was taken back to Paris.

The least movement was so painful to him that he could only bear the journey by having his head supported by Madame Letellier's hands.

Dr. Louis tried blisters, sage, quinine and morphia, without any effect. To relieve his pain, M. Letellier took so much morphia as to fall into an alarming state of torpor.

On the first of October, 1855, we found the sick man in a state of extreme agony; he had not taken any morphia for two days, and suffered constantly.

On the same day, we began to use the valerianate of ammonia, two tea-spoonsful during the day in a half glass of eau sucrée. That night he had a little rest.

By continuing the use of the valerianate without increasing the dose, the patient was able to get up at the end of five days. On the ninth day he walked out to take a bath; he no longer felt any pain except in his neck and the back of his head; his nights became comfortable, his abilities and aptitude for business entirely restored.

Finally, from the 24th of the same month we discontinued our visits.

We met him again recently, and he tells us that he has had some slight twinges, which are at once dissipated by a spoonful of the valerianate.—*La Revue Médicale Francaise et Etrangére.*

BIBLIOGRAPHICAL NOTICES.

Human Physiology, Statical and Dynamical; or, The Conditions and Course of the Life of Man. By JOHN WILLIAM DRAPER, M. D., LL. D., Professor of Chemistry and Physiology in the University of New York. *Illustrated with nearly 300 wood engravings.* New York: Harper & Brothers. 1856.

The position held by Professor Draper, as a distinguished original investigator, as well as author and teacher, has led us to expect much from this volume; and, in the anticipation that it would possess decided merits, we have not been disappointed.

It is "the text of the Lectures on Physiology which the author

has given for many years in the University," to "classes of many hundreds." No haste has, therefore, enfeebled its preparation; but it contains the deliberate views of a teacher, experienced both in the matter of his study and in its expression. We may view it, fairly, in a three-fold aspect; first, as a Text book; secondly, as a Compend of the Facts of the Science discussed; and lastly, as a Philosophical or Methodological contribution to Science, in a wider sense.

As a text book, we should think it remarkably well adapted for success. Its general style is, in comparison with the others accessible to students in this country, especially agreeable and interesting; more so, perhaps, than that of any other now in use. Valentin's work, to which the treatise of Professor Draper bears, in style and manner, the most resemblance, is too cumbrous; and the same may be now said of Carpenter's, notwithstanding their unsurpassed chasteness as well as accuracy; they have become *cyclopedic*. And, in the absence of either dryness or superfluity, no other treatise can in our view, be held superior to this. Its value is heightened by the addition of a number of new and good illustrations, to those now common to all such works. Of the 296 wood engravings scattered through Dr. Draper's volume, 68 are named as "by the author;" a goodly number in these days of compilation. A number of them have been obtained by the aid of *microscopic photography;* the process having, as it is stated, "been so far improved by the author as to be rendered very available for these uses." Nearly all of the copied engravings, also, have been obtained through the intervention of photographs.

The work is divided into two Books; the first of which, under the head of Statical Physiology, comprises twenty-five chapters, on the "Conditions of Life"—giving the history of all those functions usually called organic (except reproduction), and also those of the organs of sensation, of the brain, and of animal motion. The second book contains, as "Dynamical Physiology," eight chapters, on "the Course of Life." Development, Reproduction, Sleep and Death, and the Influence of Physical Agents upon organized beings, fill all of these but the concluding chapter, which is upon "Social Mechanics;" viewing history as a corollary to Physiology. In regard to the

method of this whole arrangement, we shall have a few words to say presently. Let us, first glance through the volume, to see how it fulfils its second indication or purpose as a Compend of the essential facts of the Science of Physiology.

In this regard, we shall find it well brought up to the time, and containing a considerable amount of matter which is comparatively new and original, both in opinion, and in observation and experiment. By the omission, for the most part, of long argumentative discussions, and of historical accounts and references upon mooted points, the author has obtained space, in a volume of moderate size, for more, of real importance, than is often compressed into the same bulk without the loss of completeness.

The opening subject of the work is that of the conditions of life—"air, water, and combustible matter." In discussing the "receipts and waste" of the body, besides the facts ordinarily stated, tabular views are given; as Physiological Standard Tables, of the diurnal ingesta, secretions and excretions of a man of the weight of 140 lbs.; and also of the same items compared to the weight of the body as 1000 parts.

The history of Digestion, given in chapters III and IV, is that which is warranted by the latest observations—including those of Lehmann and others, upon the usual presence of lactic acid in the gastric juice, recently confirmed (as reported in the *Examiner*, July, 1856) by Dr. F. G. Smith, in his experiments upon Alexis St. Martin.

In the chapter upon Absorption, it is stated that "the action of the villus is doubtless more complicated than is generally represented; for the organic fibre-cells it contains give to it the power of executing *rythmic motions*." Peyer's glands are regarded by Dr. Draper as belonging to the absorbent rather than the digestive apparatus; many lacteal vessels proceeding from them, "as may be very plainly observed during digestion." The elaboration of fibrin from albumen, and the development of the chyle-corpuscles (embryos of the blood-cells) are considered to be the two principal purposes of the absorbent system, including its glands. The view of J. Simon and some other British physiologists and pathologists, that fibrin is an

effete and not a plastic substance, is objected to by Professor Draper.

The subject of capillary attraction has long been a favorite one with our author; especially as to the mode in which it affects the movement of the blood. The following propositions are laid down:

"1st. If the force of attraction of the particles of a solid for those of a liquid, be not equal to *half* the cohesive force of the latter for each other, the liquid will refuse to pass through a pore of that solid substance, and, in a capillary tube consisting of it, will be depressed below its hydrostatic level.

2nd. If the force of attraction of the particles of a solid for those of a liquid exceeds *half* the cohesive force of the latter for each other, but is not equal to the whole force, the liquid will pass through a pore of that solid substance, and, in a capillary tube of it, will rise above its hydrostatic level.

3d. If the force of attraction of the particles of a solid for those of a liquid exceeds the whole cohesive force of the latter, chemical union between them ensues."

Endosmosis and exosmosis are correctly explained as identical with capillary attraction, except in the additional power given by the presence of a fluid *at the ends* of the fine tubes which the membrane or porous septum represents. The law commonly stated, that the stronger current in endosmotic transmission is always from the lighter to the denser liquid, is denied. "Its error is readily shewn by many simple illustrations. The relation of specific gravity has nothing whatever to do with the action." (p. 107.)

The contribution rendered by Professor Draper to the theory of the circulation of the blood, through his experiments and observations upon capillary phenomena, is familiar to every physiological student. It may be interesting, nevertheless, to recall, in his own language, the principal basis of the opinion, associated with his name, that capillary power is *the* agency by which the blood is circulated through the whole frame, as the sap is moved in plants. It is (so far as we can quote without the diagram) as follows:

"If two liquids communicate with one another in a capillary tube, for the substance of which they have affinities of different intensities, movement will ensue—the liquid having the highest affinity will occupy the tube, and may even drive the other before it. The same effect

will ensue in a porous structure. Thus, let *b*, *b*, be a capillary tube of any kind, which is occupied conjointly by two liquids, *a* and *v*, meeting each other in its middle; *a* having a high and *v* but little affinity for the substance of which the tube consists, *a* will occupy the tube, pressing out *v* before it. Of course, it is to be understood that the liquids *a* and *v*, respectively, communicate with reservoirs that can furnish them a necessary supply. The various phenomena described under the designation of endosmosis are experimental illustrations of the same kind. Now it is precisely relations of this kind that are observed in the case of the circulating and nutritive juices of all organic beings." "The juice which is about to nourish a part has for that part a certain affinity, but, with the accomplishment of that nutrition, the affinity is at once lost. Thus, for instance, in the systemic circulation, the parts to be nourished have a certain affinity for the arterial blood; they take from it whatever their purposes require, and, that done, the relation at once ceases; the blood, become venous, has lost its hold upon them, and is pressed off. We may conveniently describe this effect as a pressure of the unchanged upon the changed liquid."

This principle, variously applied, is supposed by Dr. Draper to account for the *whole circulation* of the blood, with some trifling aid, only, from the heart, equalized in turn by the arteries.

It is impossible to acknowledge so much as this, notwithstanding the importance of the principle, (with some slight modification) as explaining *one* of the moving agencies of the circulation.

The modification which reflection suggests as necessary, is, we think, easily made intelligible.

Let us recall the three laws of capillary action, quoted upon a previous page, and, in view of them, compare the experiment of the tube and two liquids, with the circumstances of blood-circulation and nutrition.

There is this difference; that, while, in the tube experiment, no combination is supposed to take place between the liquid and the walls of the tube, in the case of the blood flowing through a capillary vessel, we must notice that—1st, the governing attraction is *not* for the walls of the vessel, but for something *outside* of them; and 2nd, that a *combination takes place* between the particles attracting and those attracted. The latter particles are therefore being constantly *withdrawn* in a current or currents through the sides of the vessel.

The case of the experiment with the tube and fluids comes

under the second of the laws laid down by Prof. Draper on p. 105 of his work; the attraction of the particles of the tube for those of *one* of the fluids exceeding *half* the cohesive force of the latter, but not equalling the whole of it; and thus causing it to rise above its hydrostatic level, and even carrying the other fluid before it.

But, in the case of the blood-vessel whose plasma is passing out, laterally, for nutrition or secretion, a *double* capillarity is observed; that of the *capillary tube* itself, and that of its *permeable walls*. The chemical combination, also, which is a part of the nutrition, or secretion, &c., occurring, makes it a case under the 3d law, above alluded to, when, the force of attraction of the particles engaged exceeding their whole cohesive force, chemical combination, instead of hydrostatic impetus, ensues. The traction is lateral, not impulsive. The reticular arrangement of the capillaries varies incessantly its direction, it is true; but still, in every case, the power exerted is only, in regard to the current in the vessels, sufficient to act, (as lateral suction might do, to a less degree, by removal of pressure), in facilitating the approach of particles of the blood *to* the capillary region in which extravasation is taking place. No important addition can thus be afforded to the impetus of the venous circulation returning toward the heart. We are anxious to make this, if possible clear; since there is a certain amount of boldness in thus proposing to modify a doctrine, now so generally accepted. A rude illustration may elucidate our meaning further.

Suppose that, in the middle of a passage-way between two parallel ropes, two files of men meet each other, going in opposite directions. Suppose, also, that of those going eastward all use traction by the ropes; while those who go westward are all *hand-bound*. There will then be a case somewhat like that of the experiment of Dr. Draper, with the tube and two liquids; one of which, having attraction for the walls of the tube, pushes the other before it.

But if, on the other hand, we suppose that, instead of traction, *extraction* be used by other men *outside of the ropes*, lifting out of the passage one after another of those who go in one direction, we have then the case of nutritive affinity clearly

represented: the effect upon the opposing current or file, being one of *arrest*, only, not of backward propulsion, to any extent. The relative importance of the power derived from such a source as one of the mechanical agencies of the circulatory system, would seem to be diminished, not at all destroyed, by these considerations.

We might say more. Dr. Draper is of opinion that "the arterialization of the blood in the lungs is the cause of the circulation in man. I consider the circulation as the consequence of respiration." Its subordinate actions are "all referable to one primordial act, and that is, the exposure of the blood to the air."

And yet, it is understood, that the oxygen we breathe is not permanently, but loosely, if at all, combined in the blood, but is carried by the red discs or blood-cells, and freely given off to the tissues. In the words of our author, "the generation of carbonic acid is commonly referred to the soft tissues. Doubtless much originates in this way." "That nothing analogous to combustion," (that is, active chemical combination,) "occurs in the lungs, is proved by their temperature, which is not higher than that of other parts of the system."

And yet, we are called upon to believe that the affinity of the blood in the capillaries of the lungs for oxygen (although not sufficient to produce any active combination,) is, at last, *the cause*, not merely one of the contributing causes of the circulation! Again, the portal circulation is accounted for, with all its exceptional character, by a chemico-dynamic relation between the liver and portal blood. Yet Dr. Draper himself holds that "the bile simply transudes from the blood, and that the cells of the lobules have *no special relation to it* beyond this, that it oozes past their interstices, or, perhaps, *by physical imbibition*, finds access to their interior." Certainly there must be here some inadvertency, if not inconsistency.

The author states, that the action of the heart is "limited to *bringing the blood to the arterial origin of the capillaries*, and beyond that point its action may be considered not to extend." (pp. 145–7.) Yet, the absolute force which the left ventricle exerts for the propulsion of the blood into the systemic arteries, is mentioned (upon p. 139) as being equal to 13 pounds;

that of the right ventricle about half as much. *What becomes* of all this force? It certainly cannot be all spent in dilating the arteries, which, by their muscularity, would re-bound without loss, but rather with gain of power. The blood, moreover, is not *to be brought*, with each impulse of the heart, to the capillaries, but, being already there, the *column only* has to be impelled. This has been overlooked.

In fine, the many phenomena adduced (upon p. 144) to show the necessity of a capillary power, apply equally well, in most cases, to that of an *arterial* impulsion; and are to be accounted for by the joint influence of *both* these systems acting as supplementary to the heart. We must be understood as not denying the existence of a modifying power exerted upon the circulation in the capillaries, dependent upon the relations of the blood to the tissues; but only as deprecating the exaggerated estimate of its *relative* importance, into which Dr. Draper, with many other recent writers, seems to have fallen.

The chapter upon Respiration presents, in addition to the ordinary facts, some curious statements in regard to interstitial movements exhibited by *solids*, as well as by liquids and gases; besides some very interesting investigations by the author upon the occurrence of gaseous diffusion, without diminution, under a pressure of fifty atmospheres; and experiments upon the amount of watery vapor of respiration, by his son, Dr. J. C. Draper, of New York. It was proved, by these last experiments, that the quantity of water exhaled varies directly with the *number* of *respirations*; not being diminished in proportion, as is the case with carbonic acid, when they are increased in rapidity.

The influence of diurnal variations of temperature upon the condition of the body, in health and disease, is noticed in the succeeding chapter upon Animal Heat.

Much stress is also laid upon a probable mode of action of the nervous system, which has not received all the attention it deserves. It is that of its controlling, not only animal heat, but many other changes in the living body, by affecting the *allotropic state* of its component elements, whether simple or compound. Berzelius and Faraday have made great account of allotropism in inorganic bodies; it remains to be definitively applied to the organic.

"There are some well-known facts in natural philosophy, which throw a flood of light on this obscurity. If a piece of pure zinc be placed in a glass of acidulated water beside a piece of copper, so long as the metals are kept apart, no action whatever ensues; but if a conducting thread is laid from one to the other, the zinc instantly begins to oxidize, clouds of hydrogen gas bubbles rise from the copper, and the thread becomes at once red-hot and magnetic. On lifting the communicating thread, all these actions cease; on restoring it they instantly recur. We think we explain them by saying they are all due to the decomposition of water by the zinc. But why was the zinc passive when alone, and why did it assume this activity when merely touched by another metal? Does not all this serve to show that substances may be, as it were, in a quiescent state, and, on the application of what may, perhaps, seem the most insignificant cause, may suddenly assume activity, and forthwith satisfy their chemical affinities?

"I have endeavored to prove that allotropism is the true cause of many of the obscure facts which we meet with in the animal mechanism." "We may conclude that, so far as tissue destruction is concerned, the nervous system possesses a governing or controlling power; that, by keeping parts in states answering to the passive and active conditions of inorganic chemistry, it can suspend the action of the respired oxygen or permit it to take effect." "It is also possible that these states of activity or passivity are likewise impressed upon the oxygen itself, since that gas is no exception to the rule; it also exhibits allotropism. Its passive state is Priestley's oxygen, its active is ozone."

These suggestions are important, and flow naturally from many recent investigations. Would not the term *dynamization* be, perhaps, more convenient than "active allotropism," or allotropic states? Ozone is dynamized oxygen. The nervous system gives a dynamic energy to particles otherwise inert. We may conceive that in the blood, this intensity of the affinities of all its elements may be wrought up to its highest pitch; and that in this consists part of the effect of the assimilating organs. Portal blood is liquid food; arterial blood, fluid tissue.

The view of Dr. Draper in regard to the action of the liver in the secretion of bile, has been already alluded to. Upon the broad subject of secretion, his statement is this: "Perhaps we should not be very far from the truth if we considered all those secretions in which the materials are in a state of retrograde metamorphosis, or in a descending career, as arising by mere filtration, and those which are ascending to a higher grade as due to cell agency; between the two there being an intermediate class, the phase of which is stationary, and in which cells may or may not be necessarily involved; as, for instance, the trans-

mutation of one fat into another, or the preparation of sugar from the albumenoid bodies." It is added, that "the more profoundly we study the composition and constitution of the secreted fluids, and the more accurately we understand the function of secretion itself, the less are we disposed to invoke the agency of cell-life, and to rely the more on the ordinary mechanical act of strainage."

Bile, it is said, is increased as to its amount of fluid by calomel, but its solid constituents diminished. Carbonate of soda diminishes both. These statements are not ordinarily made, and may not be universally acknowledged. The fact that the resinous matter of bile is found in the stools after mercurials, although not commonly present, has been stated by several observers.

The fact of the manufacture of *sugar* by the hepatic cells, is fully admitted. And yet, sugar, being vegetative, can hardly be called a product of progressive metamorphosis. Why then, might not similar cells *produce bile*, perhaps as a complementary result? It is, indeed, supposed, by Dr. Draper, that the coloring matter of the bile is derived from hæmatin, the blood-cells being disintegrated in the liver and in the spleen.

As to the functions of the latter organ, the views of Prof. Kölliker are adopted, and no allusion is made to the admirable essay of Mr. Gray, in which, results are stated which were obtained by experiments and observations on a large scale, and with the utmost care, proving that the function of the spleen is to regulate both the *quantity* and *quality* of the blood.

We cannot but think it straining a theory not too firmly established, to class the emission of milk from the mammary glands among the *excretions*, in immediate succession to the consideration of the urine.* Not to dwell long upon this point, if even casein (as kyestein) and the other ingredients of the mammary secretion are pre-existent in the blood (which is denied), how could *corpuscular forms*, milk corpuscles, exist in it, without at least some agency from *cell-life?* They certainly cannot be *filtered* out of the gland.

The existence of nucleated cells upon the Malpighian tuft or

* Chap. XII.

coil has been proved by Dr. Isaacs; a drawing from whose paper, read before the New York Academy of Medicine, is given, confirming Bowman's views of the structure of the kidney.

Passing by a chapter on "Nutrition and Decay," as more connected with the second Book, we come next to the Functions of the Nervous System.

Some exceedingly interesting remarks are made upon the supposed action of the vesicular nervous centres as "registering ganglia," and as reservoirs of force.

"There cannot be a doubt that the registry of impressions involves an actual structural change in the ganglion, which is of a permanent character. These changes may be rudely and imperfectly illustrated by experiments, such as I published years ago, of which the following may be taken as examples: If, on a cold, polished piece of metal, any object, as a wafer, is laid, and the metal then be breathed upon, and, when the moisture has had time to disappear, the wafer be thrown off, though now upon the polished surface the most critical inspection can discover no trace of any form, if we breathe upon it a spectral figure of the wafer comes into view, and this may be done again and again. Nay, even more; if the polished metal be carefully put aside where nothing can deteriorate its surface, and be so kept for many months, (I have witnessed it even after a year) on breathing again upon it, the shadowy form emerges; or, if a sheet of paper on which a key or other object is laid be carried for a few moments into the sunshine, and then instantaneously viewed in the dark, the key being simultaneously removed, a fading spectre of the key on the paper will be seen; and if the paper be put away where nothing can disturb it, and so kept for many months, at the end thereof, if it be carried into a dark place and laid on a piece of hot metal, the spectre of the key will come forth. In the case of bodies more phosphorescent than paper, the spectres of many different objects which may have been in succession laid originally thereupon, will, on warming, emerge in their proper order.

I introduce these illustrations for the purpose of showing how trivial are the impressions which may be thus registered and preserved. Indeed, I believe that a shadow never falls upon a wall without leaving thereupon its permanent trace—a trace which might be made visible by resorting to proper processes. All kinds of photographic drawing are in their degree examples of the kind."

The chapter on the Brain appears, to us, one of the least satisfactory in the volume; considering the amount of knowledge added to our stock since the days of Hartley. It would seem that a certain dread of "metaphysical speculations" has deterred the author from attention to inquiries which might rather with correctness be called "physchological inductions."

An implied assent is given to the doctrine of Gall and Spurzheim, "that particular regions of the brain are devoted to special functions, and that by an inspection of the exterior of the cranium mental peculiarities may be detected." No opinion is given (as might have been expected) upon the value of the inductive evidence asserted by the phrenologists, as to the detailed localization of these special cerebral organs. We have only the rather loose (for a volume of "Positive Science") allusion, as follows, to one series of them: (p. 546.) "It *has been asserted*, that the moral and intellectual peculiarities which woman presents when compared with man, are distinctly traceable to the phrenological preponderance of the moral over the intellectual portions of the brain."

The opinion of Descartes (advocated also by Brougham, Carpenter, and others,) that insects are, even in the most complicated of their instinctive actions, automatic, is disputed. We have no hesitation in accepting the views of the highly distinguished authorities above named upon this subject. We are told by Professor Draper (p. 609), that "they are not automata, since they are under the influence of the cephalic ganglia as the registers of past impressions." A just view of comparative psychology proves that this does not alter the case, in regard to automatism. Nor does the exercise of intelligence. Instincts are either executive or impulsive; in the lowest animals the former; in the higher, the latter, almost alone.

We may refer to the admirable chapters on the Nervous System in Dr. Carpenter's Treatises (Human and Comparative Physiology) for the best discussion of this subject extant. There it is proved, especially upon the basis of Dr. Laycock's investigations, that our ideas of automatic or reflex action must not be limited even by instinct. *Cerebro-reflex* action occurs, even in man, to an extent, and with an importance, which only needs suggestion to be confirmed by every one's experience.

Looking upwards, through the animal series, from the amæba toward man, we shall find (as true positive philosophy throws the *onus probandi* upon the side of *volition* as an improbable postulate) that a careful exclusion will leave no evidence of actions in the invertebrate, no, nor in the vertebrated animal, which cannot be ascribed to automatic influences, acting through

a complex muscular, ganglionic, or cerebral mechanism, until we come to *man* himself. And, in man, the evidence of a different power, of choice, of will, comes from *consciousness*, not from observation. Looking upon other men, we would have no proof of it. Man *is* an automaton, not only as regards his spinal system, but so long as he is governed, as many men are, by unrestrained instincts, propensities, ideas: while, in the highest efforts of genius, especially in its æsthetic developments, there are evidences of an automatic nature, although under the control of the will, as clear as in the simplest case of reflex action. We dwell upon these things, not because they are not familiar, but to regret that Dr. Draper has preferred to ignore them; and to express the opinion, that they are of great importance, in relation to that philosophy of accountability and morality, which he has repeatedly spoken of in terms of the highest respect.

Passing on, we find in the work before us a very clear and interesting account of the physiology of the organs of sense. The general view taken of the "triple function" of the internal acoustic apparatus, is, that 1st, the *drum* is for the measurement of intensity of sound; 2d, the *cochlea* for the recognition of wave-length (pitch?); 3d, the *semi-circular canals* for the appreciation of the quality of sounds. The last of these conclusions is the least satisfactorily established. The argument would seem to be at least equally strong in favor of either of the other hypotheses; the older one, that the semicircular canals have relation to the direction of sounds,—or that of Roget and Professor Jackson, that they serve to exhaust or arrest *waste* undulations, and to prevent their repercussion upon the ear.

As to the eye, several very interesting suggestions are discussed. Thus,

"There has been a difference of opinion as respects the actual screen upon which the images form. Some of the early optical writers regarded the black pigment as being that receiving surface, an opinion which has been universally abandoned, the function having been of late attributed to the retina, but, as it appears to me, on totally insufficient grounds. The arguments against the retina, both optical and anatomical, are perfectly unanswerable. During life it is a transparent medium, as incapable of receiving an image as a sheet of clear glass, or the atmospheric air itself; and its sensory surface is, structurally, its exterior one; that is, the one nearest the choroid coat. But the black pigment, from its perfect opacity, not only completely absorbs the rays of

light, turning them, if such a phrase may be used, into heat, no matter how faint they may be, but also discharges the well-known duty of darkening the interior of the eye, and therefore preventing indistinctness from the straying of the rays of light. Perfection of vision requires that the images should form on a mathematical superficies, and not in the midst of a transparent medium. The black pigment satisfies that condition, the retina does not."

We have also a theory of the *rationale* of vision, as a function of the retina. On the strength of his own experiments, and those of Melloni, the author states that, although the prism indicates otherwise, the yellow or brightest ray is the hottest, and that the warming powers of the others follow in the order of their luminous intensity. And, in his opinion, the act of vision commences in this local disturbance of temperature. The club-shaped particles of Jacob's membrane are tactile organs, which communicate to the sensory surface of the retina the condition of temperature of the black pigment. But this communication of a variety of temperature implies a variation in the waste and repair of the retina itself; the physical change being thus converted into a physiological equivalent in the metamorphosis and destruction of a nervous tissue. Now the essential condition of the chemical activity of a ray of light, whether for photographic or calorific effects, is absorption. A surface like the black pigment is capable of absorbing all the rays alike. The impression arising from the disturbed condition of the retinal vesicles is carried by the optic tubules to the chiasm of the optic nerves; and, we might add, to the tubercula quadrigemina, as the central ganglia of sight. Objections may be raised to parts of this theory; but the problem is too abstruse for the present article. Professor Draper recognizes the undulatory theory of light upon another page, in saying that "it amounts only to a different mode of stating the same effect, since, as I have shown (Lond. and Edin. Philosoph. Magazine, May, 1851), all chemical changes accomplished in material substances are occasioned by the establishment of vibratory motions therein, and Ampère has already demonstrated that all the phenomena of heat may be explained upon the doctrine of the vibrations of the constituent molecules of bodies."

A somewhat analogous theory to the above is presented in regard to the physiological action of the muscular fibre; to the

effect that "muscular contraction is the *physical result* of *muscular disintegration.*" Rhythmic, as well as other contractions are thus explained, upon a principle which we must now pass by. One or two yet more important points remain for discussion, the omission of which would be inappropriate.

We have observed, that the whole domain of Physiology has, by our author, been mapped out into Statical and Dynamical. We are informed that, since physiology is to be viewed as a branch of natural philosophy, much advantage is to be expected, both present and future, from introducing, in this manner, the methods familiar to writers on mechanics.

But, we cannot think, a physicist would see, without surprise, upon a page connected with *Statical* Physiology, full descriptions of the mechanism of *locomotion*, standing, walking, and running, while, upon the opposite one, a *Dynamical* series of chapters begins, of which one is wholly occupied with *Sleep* and *Death.* The terms, it would seem to us, though we say it with deference, lack the accuracy which should recommend an innovation of this kind, otherwise than as a mere novelty. Care is especially required in transferring the method of one science to another, since *similarity* of treatment, only, not identity, is possible, where the subjects are not the same. "If the case was clear," writes the astute Comte, "in regard to the intrusion of the geometers into physics, it is yet more so with regard to the intrusion of the physicists into Biology; on account of the *more essential difference in the nature of the two sciences.*"

Is not Statical Physiology simply *Anatomy*, the study of the body as it *exists*, not as it *acts?* It is true, that a *movement*, a career, occurs in growth, and may be called dynamic; but this is true *also*, and more "in the method familiar to physics," in the movements of the body in mass, and in the reactions which belong to the properly animal functions of the organism. An ambiguity is thus created, rather to a disadvantage than otherwise.

De Blainville's subdivision of Biology into zootomy, or anatomy,—zootaxy, or classification, and dynamical biology or physiology, is a better one: the latter being then divisible into the functions of Organic and those of Animal Life. The collateral advantages sought for by Professor Draper could be thus

as fully obtained, at least, without any doubtful terminology. We are the more surprised at the classification he has chosen, from the manner in which it stands related to the views he holds of Life and of Development.

These are the purely physico-chemical views. Not Boerhaave, Lamarck, Lehmann, or Moleschott, could go farther. Although provisional use is made of the "elementary conception" of a "plastic power," and its "apparent evidence, from a superficial consideration,"—this is corrected afterwards, by an explicit denial of the existence of anything more than the ordinary physical agencies of inorganic matter.

We ought to justify such assertions by free quotation; and this is easily done. On page 500, we have it as a general statement, that

"Structural differentiation is to be received as the cause of functional differentiation. The former, in every instance, arises from the changed circumstances under which cells are being generated."

Again, (p. 253):

"It appears to me that many instances of nutrition, growth, and development can only be explained by admitting, as a great and fundamental principle in physiology, that, the primordial germ being in all instances alike, its mode of development will depend on the physical agents and conditions to which it is exposed."

The following might well please Lamarck, or the author of the "Vestiges of Creation." (p. 506).

"Starting from a solitary cell, development takes place; and, according as extraneous forces may be brought into action, variable in their nature and differing in their intensity, the resulting organism will differ. If such language may be used, the aim of nature is to reach a certain ideal model or archetype. As the passage toward the ideal model is more or less perfectly accomplished, form after form, in varied succession arises. The original substratum or material is, in every instance alike; for it matters not what may be the class of animals or of plants, the primordial germ, as far ar investigation has gone, is in every instance the same. The microscope shews no difference, but, on the contrary, demonstrates the identity of the first cell, which, if it passes but a little way upon its forward course, ends in presenting the obscure cryptogamic plant, or, if it runs forward toward reaching the archetype, ends in the production of man. The diversity of form that is eventually presented, depends then, not upon the constitution or aspect of the primitive cell, but upon the influence of the many surrounding agencies to which it is exposed." "Not only is the primordial cell in all instances the same, but the first stages of its career are, in all instances,

identical; and this, whether we consider it in the lowest or the highest cases, belonging either to the vegetable or the animal kingdom." "This career of development applies equally to the case of any individual animal, or any race of animals. Thus, man himself, in succession, passes through a great variety of forms, from the condition of a simple cell; these forms merging by degrees into one another, the form of the serpent, of the fish, of the bird, and this not only as regards the entire system, but also as regards each one of its constituent mechanisms—the nervous system, the circulatory, the digestive."

We have, therefore, each one of us, had a *narrow escape*, depending "altogether on extraneous circumstances," from turning out "an armadillo, a flying-fish, or a stag-beetle!" We need hardly stop now to say, that those who have contributed the most to the science of embryology as it is, make a different statement from the above: to the effect that, in every case, the law of development from the *general to the special* is the only one that can be affirmed; no real correspondence ever existing, for example, between the embryo of Man and the completed type or form of any other class, in any of the systems which have been referred to; the real resemblance being only in the *embryoes* of the different classes and types, in the gradual process of differentiation.*

Looking farther into Dr. Draper's views, we are met, unexspectedly, by a somewhat different train of thought. He remarks (p. 549), that

"That active agent which was first laid in a fold of the germinal membrane was not annihilated when its type of life was changed to placental and therefore aquatic respiration. It withstood the shock when again, after a due season, it was suddenly made to breathe the air. Arrived at the mature condition, the nature of every one of its organs has changed. Through all these losses and changes, the immaterial principle has passed unchanged, and even gathering strength. In the broadest manner that a fact can be set forth, we see herein the complete subordination of matter, and the *enduring character of spirit.*"

It will be hard not to confound this with ultra vitalism: or rather, with the doctrine of the "unconscious soul," now held by Dr. Laycock, and approved even by the philosophical Morell. But, let us look back to our former quotations. Coming to the point, in all earnestness, we ask *how about cabbages?* If the agent, or spirit, "is *laid in the primordial cell*," identical exactly for all species, animal or vegetable, and depending upon "external

* Vide Carpenter's Princip. Com. Physiol., Am. Ed., p. 125.

conditions" only, for its development into the one type or another, *what becomes of the spirit*, when these conditions favor a lower instead of a higher type? That we are not mistaken in Dr. D.'s meaning, as regards the identity of the "immaterial principle" alluded to with the soul of man, is shown upon p. 25, where it is said to be an important duty with the physiologist,—regretting the use of such terms as "mind, intellect, vital principle, spirit," which confuse the popular ideas,—to prevent, as far as he can, such expressions from "frittering away the great truth that, as there is but one God in the universe, so there is but one spirit in man."

In the discussion of the subject of Reproduction, the same error (in our belief) has polarized the judgment of our author to some degree. Ambiguity appears to be created by the mode of use of the word "individuality." *Unity* would have been a better term; which is compatible with *dividuality;* as illustrated in multiplication by *gemmation*, which is *always exhaustible.* This is acknowledged by Dr. D., in accepting Carpenter's modification of Steenstrup's views. The organism *begins as a unit*, and differentiates;—into few cells or a million,—like or unlike,—dissevered or together,—but still always (in the language of the work before us) "expanding from one common or single centre;" not built up from foreign materials imposed from without: unity first, multiplication afterwards; and this in all cases with a limit, until renewed by a generative reproduction.

The idea conveyed in the terms "plastic power" and "vital force" is, as we have seen, especially objected to by Dr. Draper. We have not space now to go into an elaborate disquisition upon them. It may be said, however, that those who now authoritatively use the latter, and some modification of the former, (as Liebig and Carpenter), do so not in a "metaphysical sense," but in one entirely compatible with the analogies of physics, and the methods of natural philosophy. The expressions of the work of Dr. Draper himself would justify them. Why else does he devote a whole Book to *dynamical* physiology, whose topic is simply the *career* of an organic germ—if that career has no governing and especial influence of its own? Discarding, as modern science does, the fiction of "entities"—and granting with Dr. Draper that the course of life, the motion of develop-

ment, is not an "agent," but an "operation,"—it is yet a peculiar operation. If there is reason for designating (however, provisionally) by the name of *heat*, the physical source of certain expansive and other movements of matter,—by that of *light*, that of certain others,—and of gravitation, of others,—we must allow that there is the *same* reason for differentiating that power which affects the mode in which other physical agents react upon the animal or vegetable germ, to so important a degree as life is known to do. We may accept the phrase "momentum of development;" a momentum, however, never exhausted until death. We must object to the term "self-origination" as applied to the organic cell (p. 494) in any case, as quite unphilosophical. But the physical illustrations adduced by Dr. Draper for a somewhat different purpose, may be made to serve ours equally well. "The career of an organism recalls the flight of a heavy projectile, thrown upwards;" and we give the name of life-force to its impulse, derived, no doubt, from an "antecedent impression." Plants and animals are, in truth, "nothing more than temporary states" of material, transitory results (like the flame of a lamp) of some "original impression," that of parentage, acting with the external agencies of nature. In the words of M. Comte, however,* we must never forget the "dualism" between the aggregate of external actions, and the action of the organism itself: the organism and the medium being both indispensable factors in a true dynamic physiology.

Error exists, it appears to us, in the assumption, too commonly made, that, because there is no *perceptible* difference between germ and germ, of different species, we can therefore predicate that no difference exists. We have no graduated *vitometer* to measure life-temperature, to small degrees, or to estimate germinal capacity. We must consider ourselves very much in the dark, therefore, upon the whole subject. A dogmatic negation would be as great an evil as a "middle aged" hypothesis. The "positive" authors, of late times, do not always remember this. They decry old theories; but they invent new ones.

Dr. Draper has ranged himself under the flag of the "new"

* Positive Philosophy, translated by H. Martineau; p. 357.

Positive Philosophy. We are entitled, therefore, to regret, that we are unable to see any advantage the world has gained from its advent in the 19th century,—not ascribable, in reality, to the "antecedent impression" of Lord Bacon in the 16th. If it differs from this in anything, it is to a disadvantage: introducing a sort of rule of the *guillotine;* attempting to "purify the republic" of science by the removal of all suspected ideas; whereas the true remedy for all evils, social or scientific, is to be found in the due gradation of authorities, not in their destruction. If the hill of Sisyphus be the acknowledged type of all human efforts after absolute knowledge, we may admire the philosophic mind which accepts with calmness the necessity; but not the defiant spirit which, while it denounces all that is past, utters an anticipatory "eureka" with every breath.

We have no such animadversion to make on the work before us; but we may confess that some sentences in the Preface gave rise to these reflections, by a not unnatural suggestion. Thus:

"We live in a period of difficulty. Metaphysical Philosophy has lost its hold upon the human mind. The uncertainties, contradictions and emptiness of the English, Scotch, French, and German schools are manifest. Already the belief is wide spread that their barrenness of result and consequent worthlessness are the necessary incident of their method of investigation, and that we must look to some *wholly new* system as a guide to the truth on the topics they have had under consideration. That guide is Positive Science!"

To aid in removing Physiology,—"which, alone, of all the great departments of knowledge, still retains the metaphysical conceptions of the Middle Ages,"—from Speculative and attaching it to Natural Philosophy, is stated to be one of the great objects of Dr. Draper's work. But, may we not respectfully ask, are not Valentin and Lehmann, Longet and Bernard, Owen and Carpenter, Agassiz and Burnett, and scores of others, already cultivators of *positive physiological science?* If so, the removal would seem to be already accomplished; certainly is not now to be begun.

Let us, in conclusion, glance, with great pleasure, at those chapters in the work which we have not yet alluded to, but which can be only named. They are chapters II, VII, and VIII, of Book II; containing a most *admirably* written series of observations upon the influence of physical agents upon vege-

table and animal life, including that of Man; in which, amongst many other points of great interest, the Unity of the Human Species is well maintained. We have, in fine, a concluding chapter, which would do credit to the most erudite writer upon "Social Mechanics;" comprising a Physiological view of History, from the earliest authentic dates to modern times.

Upon the whole, we rise from the survey of this work, of which, if we have "nothing extenuated," we have at least "nought set down in malice," with an impression of great power and value. No ordinary book would repay half the care and study required for even these imperfect remarks.

While, therefore, we would see, with pleasure, in a future edition, some alterations, both of arrangement and opinion, we are satisfied that "Draper's Physiology" will take an important place at once: and will add one more to the number of American text-books, which may be placed side by side with the best of those obtained from abroad. If for no other consideration, we would thank Professor Draper for his work; and thanks are also due to the publishers, for the good print, good paper, and excellent style altogether, with which it does credit to their press.

Human Physiology. By Robley Dunglison, M. D., LL. D., Professor of the Institutes of Medicine in Jefferson Medical College, Philadelphia; Vice-President of the American Philosophical Society, etc., etc. *With five hundred and thirty-two Illustrations. Eighth edition, revised, and enlarged. In two volumes.* Philadelphia: Blanchard & Lea. 1856.

It affords us much pleasure to be able to announce the appearance of the eighth edition of the above work. The very short period in which the previous issues have been exhausted, exhibits, in a very flattering light, the high estimation in which the work is held by the profession. In preparing the present edition, "no pains have been spared to make the work a complete expression of the science of the day." This statement our own examination of the work enables us to confirm; every page of it almost testifying to the author's industry in culling from various quarters and sources all that was valuable in the

physiological contributions to science of the last few years. The careful and scrutinizing spirit exhibited by the writer, when investigating mooted questions; the extensive information he possesses of general science in almost every department, and the clear and happy style in which he presents his views, render his physiology one of the most reliable and attractive works in our language.

To the practitioner and general reader, we can heartily recommend it as an excellent resumé of the present state of physiological science. As a text-book for the student, we think it has no superior in our language, and for this object we presume it was chiefly if not expressly written.

New Remedies: With Formulæ for their Preparation and Administration. By ROBLEY DUNGLINSON, M. D., Professor of the Institutes of Medicine, etc., in the Jefferson Medical College of Philadelphia. *Seventh edition with numerous additions.* Philadelphia: Blanchard & Lea. 1856.

Among the new remedies (either so-called from their being lately discovered, or from their being employed in a manner unknown before) described in the present edition are apiol, caffein, carbazotic acid, cauterization and catheterism of the larynx and trachea, cedron, cerium, chloride of bromine, chloride of iron, chloride of sodium, cinchonicine, cod-liver olein, congelation, eau de Pagliari, galvanic cautery, hydriodic ether, hyposulphite of soda and silver, inunction, iodide of sodium, nickel, permanganate of potassa, phosphate of lime, pumpkin, quinidia, rennet, saccharine carbonate of iron and manganese, santonin, tellurium and traumaticine.

The authority from which his information was obtained is given by the author, in every instance, at the bottom of the page, by which means the reader, anxious to avail himself of fuller information on any subject, is enabled to refer at once to the original article. The sources of information are exceedingly various, the materials being taken from English, German, French and American authorities. Proverbial as is the author's industry, the labor he must have undergone in collecting so great a mass of facts, cannot but excite the astonishment of every one who knows how tedious and toilsome such research is. The style

in which the work is written is flowing and perspicuous, and the opinions given are marked by the author's usual judgment and caution. Independently of the information it contains, the book is an agreeable and readable one; and to all who are desirous of becoming acquainted with the properties and applications of the many new remedial agents of the last few years, we take great pleasure in recommending it.

Obstetrical Tables. By Dr. Pajot, Agrégé Professor to the Faculty of Medicine, Paris. *Translated from the French, and arranged by* O. A. CRENSHAW, M. D., and J. B. MCCAW, M. D., Richmond, Va. *With three additional Tables on the Mechanism of Natural, Unnatural and Complex Labors.* By NATHAN P. RICE, M. D., New York. Richmond: Enquirer Book and Job Press. 1856.

Dr. Pajot's is a name well known to obstetrical science, and this work, though but a summary, is, by the admirable clearness and methodical arrangement of the tables, (five in number,) a most valuable addition to our other means of instruction on some of the most difficult and dangerous obstetrical complications.

Table first is devoted to the signs of pregnancy, with the functional derangements thence arising, together with a tabulated review of the touch, palpation, auscultation and percussion as means of diagnosis.

The second table is a classification of the deformities of the female pelvis in connection with child-birth; the causes of malformation, with the indications and treatment.

The third table is a synopsis of the treatment of hæmorrhage.

The fourth is an admirable summary of the subject of turning, with the difficulties and complications attending the operation.

The fifth and last of this series is devoted to the application of the forceps, divided into three stages. First, the introduction and placing of the branches. 2d, the articulation of the blades, and 3d, the stage of extraction.

The translators have considerably enhanced the value of the work by adding three excellently arranged tables, compiled by our countryman, Dr. Nathan P. Rice, of New York, on the subjects of Natural, Unnatural and Complex labors.

Drs. Crenshaw and McCaw deserve the thanks of the profes-

sion for the great care and correctness with which they have translated and arranged these tables. Dr. McCaw's summary of the causes and treatment of post-portem hæmorrhage concludes the work, and renders the series complete in every particular. The work is beautifully got up, and, as to paper, type, &c., unexceptionable. We recommend it with confidence to students and practitioners as a useful addition to their libraries.

A Review of the Present State of Uterine Pathology. By JAMES HENRY BENNETT, M. D., &c. Philadelphia: Blanchard & Lea. 1856.

The object of this little work is a critical consideration of the several objections urged by Drs. Lee and West against the existence and pathological importance of inflamatory ulceration of the neck of the uterus. The leucorrhœa theory, the syphilis theory, the ovarian theory and the displacement theory are all of them severally reviewed, also, and found wanting. As an exposition of the author's peculiar views upon uterine pathology, the corretness of which time and experience have fully satisfied him, and, as an able argumentative discussion of the doctrines held by other writers, we heartily recommend it to our readers.

The Physician's Visiting List, Diary and Book of Engagements for 1857. Philadelphia: Lindsay & Blakiston.

The above contains an almanac, a table of signs, a table of poisons, with their antidotes, a table for calculating the period of utero-gestation, blank leaves for visiting list, memoranda, addresses of patients, memoranda of wants, and of obstetric and vaccination engagement. Copies are prepared for twenty-five or fifty patients, and, when needed, will be printed for one hundred patients. We have used the visiting list for several years past, and can confidently recommend it to the profession as an excellent diary and book of engagements.

THE MEDICAL EXAMINER.

PHILADELPHIA, OCTOBER, 1856.

MEDICAL NEWS.

MASSACHUSETTS STATE MEDICAL SOCIETY.—*Prize Question for* 1857.—The Massachusetts Medical Society is authorized, by a donation from one of its members, to offer the sum of *One Hundred Dollars* for the best Dissertation adjudged worthy of a prize on the following theme, viz:—

"*We would regard every approach towards the rational and successful prevention and management of disease, without the necessity of drugs, to be an advance in favor of humanity and scientific medicine.*"

The packet accompanying the successful Dissertation will be broken in open meeting, and the author's name announced, at the annual meeting of the Society in May, 1857.

Dissertations for the above prize must be sent (post-paid), to the Corresponding Secretary, Dr. C. E. WARE, No. 6 Temple Place, Boston, on or before April 15th, 1857.

By order of the Councillors,

B. E. COTTING, *Rec. Sec'y.*

Roxbury, Nov. 1, 1855.

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Dr. Cock has been chosen President of the College of Physicians and Surgeons of New York, in the place of Dr. Stevens, resigned. Professor Le Conte has resigned the Chair of Chemistry, and Dr. Samuel St. John has been elected to the vacancy.

—

Dr. Timothy Childs, of Pittsfield, Mass., has accepted the Chair of Anatomy in the New York Medical College, previously held by Dr. E. H. Parker.

—

A case of drowning, successfully treated according to Dr. Marshall Hall's plan, (see Sept. No. of the Examiner,) is related in the August No. of the London Lancet. The patient is stated to have not been less than from fifteen to twenty minutes completely submerged after he had risen to the surface for the last time."

As it may interest some of our readers to know where copies of the works published by the Sydenham Society may be obtained, we copy the following from a circular sent us :—

THE SYDENHAM SOCETY, OF LONDON.—List of the Society's Works, of which copies are still on hand, and from which new members, subscribing for the current year, may make a selection, on payment of an additional five dollars for any three volumes, with the exception of those to which an asterisk is affixed. Those to which an asterisk is affixed, or any other single volume, may be had for $2 50 per volume.

Sydenhami Opera Omnia. 1 vol.
Hasse's Pathological Anatomy. 1 vol.
Rhazes on the Small-pox and Measles. 1 vol.
The Works of Hewson. Portrait and plates. 1 vol.
Dupuytren's Lectures on Diseases and Injuries of Bones. 1 vol.
Dupuytren on Lesions of the Vascular System, &c. 1 vol.
Memoirs of the French Academy of Surgery. 1 vol.
Feuchtersleben's Medical Psychology. 1 vol.
Microscopical Researches of Schwann and Schleiden. 1 vol. Plates.
The Works of W. Harvey, M. D. 1 vol.
The Genuine works of Hippocrates. 2 vols.
Essays on Puerperal Fever, and other Diseases Peculiar to Women. 1 vol.
The Works of Sydenham, translated from the Latin. 2 vols.
Unzer and Prochaska on the Nervous System. 1 vol.
Annals of Influenza. 1 vol.
Romberg on Diseases of the Nervous System. 2 vols.
Kolliker's Manual of Human Histology. 2 vols. Woodcuts.
*Rokitansky's Pathological Anatomy. Complete in 4 vols.
*Hunter on the Gravid Uterus. 1 vol. folio. 34 plates, with descriptive letter-press.
Wedl's Pathological Histology. 1 vol. Woodcuts.
Oesterlen's Medical Logic. 1 vol.
Velpeau on Diseases of the Breast. 1 vol.
The Works of Aretæus, Greek and English. 1 vol.

The subscription, constituting a member, is five dollars annually, payable in advance.

Three volumes, handsomely bound in a uniform manner in cloth, gilt edged, are usually issued in the year.

RICHARD J. DUNGLISON, M. D.,
Hon. Local Secretary for Philadelphia.

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THE PROVINCIAL MEDICAL ASSOCIATIN has accomplished, very quietly, its transformation into the British Medical Association—a change, the very proposal of which, last year, threatened the disruption of the

body, and met with such decided opposition, that not a few of its members felt it to be their duty to resign connection with a Society of such limited aspirations. We are glad, however, to find that a spirit of harmony now exists in the ranks of this very important body, and we trust that its present prosperous condition will be turned to good account, and that its influence will be used in promoting the best interest of the profession, apart from petty sectional rivalries. The "Association Journal" is, of course, to participate in the change of name, and will speedily be issued as the "British Medical Journal." Its existence has given rise to much discussion from time to time, and we have always been of opinion that the funds of the Association might be more usefully employed than in the maintenance of such an expensive periodical. We must, however, do the present Editor the justice to observe, that he has successfully guided its fortunes in a very perilous juncture, and that he continues to make good use of the materials placed at his disposal. By the laws of the Association, the appointment of Editor is vested in the Committee of Council. To this, objection was made, and we think justly,—several members protesting against the responsibility of electing so important an officer of the Association being left in the hands of half a dozen persons. An amendment, which proposed that the Editor should be elected by the whole Council, was, unfortunately, lost.—*Edinburgh Medical Journal*, Sept., 1856.

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The Use of Glycerine for the preservation of Organic Bodies.—Luton states that animal and vegetable substances may be kept for a long period perfectly free from decomposition when immersed in glycerine. He also finds that it is a good antiseptic agent for injecting dead bodies.—*London Lancet.*

—

Poisoning by Chloroform.—The most extraordinary overdose of chloroform yet known was wilfully swallowed by a patient recently in London. The man drank about four ounces at one draught! Wild intoxication, followed by profound insensibility, ensued; but, after various relapses and accidents, he is now quite well.—*Lon.don Lancet.*

—

Remarkable Case.—Mrs. Julia Syles, wife of John Syles, of Blackstone, died on the 14th ult. of dropsy, from which she had suffered for five years. During that time she had been tapped upwards

of one hundred and forty times, and more than three thousand pounds of water were extracted!—*London Lancet.*

—

International Banquet to Crimean Surgeons in Paris.—On Wednesday evening, August 27, a memorable international dinner was given in the splendid dining hall of the Hotel du Louvre by the Medical profession of Paris to the French, English, Sardinian, and Turkish surgeons who served in the Crimean war, when upward of 600 people sat down to dinner. Baron Paul Dubois, the senior member of the Faculty of Medicine, and the accoucheur who attended the Empress at her confinement, took the chair. At the desert, Baron Duboise proposed the following toasts:—"The names of the medical men who died in the East," and "The Emperor of the French." Several other toasts were drank all bearing upon the medical services rendered in the late war. One of them was proposed by Sir J. Hall, C. B. A toast was proposed to the Russian doctors who had humanely taken care of our prisoners.—*Edinburg Med. Jour.*

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Air-Poison.—People have often said that no difference can be detected in the analyzation of pure and impure air. This is one of the vulgar errors difficult to dislodge from the public brain. The fact is, that the condensed air of a crowded room gives a deposit which, if allowed to remain for a few days, forms a solid, thick, glutinous mass, having a strong odour of animal matter. If examined by the microscope, it is seen to undergo a remarkable change. First of all, it is converted into a vegetable growth, and this is followed by the production of multitudes of animalcules; a decisive proof that it must contain organic matter, otherwise it could not nourish organic beings. This was the result arrived at by Dr. Angus Smith, in his beautiful experiments on the Air and Water of Towns; wherein he showed how the lungs and skin gave out organic matter, which is in itself a deadly poison, producing headache, sickness, disease, or epidemic, according to its strength. Why, if "a few drops of the liquid matter, obtained by the condensation of the air of a foul locality, introduced into the vein of a dog, can produce death with the usual phenomena of typhus fever," what incalculable evil must not it produce on those human beings who breathe it again and again, rendered fouler and less capable of sustaining life with each breath drawn? Such contamination of the air, and consequent hot-bed of fever and epidemic, it is easily within the power of man to remove. Ventilation and cleanliness will do all, so far as

the abolition of this evil goes, and ventilation and cleanliness are not miracles to be prayed for, but certain results of common obedience to the laws of God.—*Dickens' Household Words, from Edin. Med. Jour.*

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GOLDEN DAYS OF THE APOTHECARIES.—When physician and apothecary were good friends, and the physician was a man who, in the phrase of the trade—for here we must needs call it a trade—could write well, something like this was the result. We quote only one day's medicine, prescribed by a physician, and administered by an apothecary, to a fever patient. The list of medicine given on each other day is quite as long, and every bolus is found in the same way duly specified in "Mr. Parret the apothecary's bill, sent in to Mr. A Dalley, who was a mercer on Ludgate Hill." We quote the supply for the fourth day's illness :—

	August 10.
Another Pearl Julap	£0 6 10
Another Hypnotick Draught	0 2 0
A Cordial Bolus	0 2 0
A Cordial Draught	0 1 8
A Cordial Pearly Emulsion	0 4 6
Another Pearl Julap	0 6 8
Another Cordial Julap	0 3 8
Another Bolus	0 2 4
Another Draught	0 1 8
A Pearl Julap	0 4 6
A Cordial Draught	0 2 0
An Anodyne Mixture	0 4 6
A Glass of Cordial Spirits	0 2 0
Another Mucilage	0 3 4
A Cooling Mixture	0 3 6
A Blistering Plaister to the Neck	0 2 6
Two more of the same to the Arms	0 5 0
Another Apozem	0 3 6
Spirit of Hartshorn	0 0 6
Plaister to dress the Blisters	0 0 6

One day's medical treatment is here represented, as it was often to be met with in the palmy days of physic, when

> Some fell by laudanum, and some by steel,
> And death in ambush lay in ev'ry pill.

Then truly might Dr. Garth write of his neighbors, how

> The piercing caustics ply their spiteful pow'r,
> Emetics wrench, and keen cathartics scour.
> The deadly drugs in double doses fly;
> And pestles peal a martial symphony.

—*Edinburgh Med. Journ. from Household Words.*

RECORD OF MEDICAL SCIENCE.

*On Phlebitis of the Great Venous Trunks of the Neck subsequent to Labor.** By ALFRED H. M'CLINTOCK, M. D., M. R. I. A., F. R. C. S. I., Licentiate of the King and Queen's College of Physicians in Ireland; Master of the Lying-in-Hospital, Dublin, &c.

The study of venous inflammation has, of late years, deeply engaged the attention of pathologists; and their discoveries, in this field of inquiry, have already thrown considerable light upon the nature and connexion of many obscure diseases. By these investigations both the practical surgeon and physician have been largely benefited; whilst the obstetrician has derived from them the greatest possible assistance, in unfolding the mysterious intricacies of puerperal fever, and explaining many of the otherwise inexplicable circumstances and features of this complex malady.

The two nearly related subjects—phlebitis and pyemia—both still require close investigation, and offer for solution many important problems. Every new fact, therefore, in their history—every novel aspect under which either disease is seen—should be placed upon record, and so rendered available to aid in maturing our knowledge, and forming a basis for scientific generalization. Accordingly I venture to bring forward the following clinical history, as it presents some features of extreme rarity and deep practical interest.

S. D., aged 22, was confined of her first child, a boy, on March 8th, after a labor of ~~twenty-eight~~ hours' duration. The consumption of time chiefly took place in the first stage, and in consequence of rigidity of the os uteri. For several days previously to the setting in of true labor, she suffered much from spurious pains; and very considerable hemorrhage occurred immediately after the expulsion of the after-birth. From the time of delivery her pulse was remarked to be quick, above 100; and this rate of frequency did not subsequently diminish. During the ensuing week she remained in a very unsatisfactory state. She had no rigor, nor anything like a distinct accession of fever or inflammation; but the pulse was constantly 110, or upwards; the belly was tumid; the uterus large, and tender on deep pressure; and she was troubled with painful tenesmus, and frequent dysenteric stools. Nevertheless, she was cheerful, and made no complaint of uneasiness in any particular situation. On her ninth day there was an apparent improvement in every respect, the pulse falling to 96, the belly becoming soft, and bowels being moved only five or six times in the twenty-four hours. On her tenth day she drew our attention to a fulness and tenderness immediately above the inner end of the right clavicle. There was a slight tumefaction in this situation, such as might be caused by a

* Read at a meeting of the Association of the King and Queen's College of Physicians in Ireland, May 7, 1856.

simple glandular enlargement. About this time she began to complain of a short, irritating, dry cough, which used to annoy her in the evening, and for which no satisfactory cause could be discovered. A few days later a similar swelling appeared in the corresponding situation on the left side of the neck. She did not seem to experience any serious uneasiness from these tumors, and beyond fomenting with warm water, and painting with tincture of iodine, no decisive treatment was employed. Some days later—namely, on the 22d March—her fifteenth day—I detected the existence of considerable œdema at the root of the neck, and across the upper and anterior part of the chest, but more so at the left side. The diarrhœa still continued, though with greatly diminished severity; her pulse was usually about 108; and the abdomen was flaccid, and entirely free from pain or fulness.

On the 26th March the œdematous swelling of her neck was decidedly less, though her face seemed somewhat puffy, and the tenderness still remained. As she was now eighteen days confined, and seemed better, we yielded to her own urgent request for leave to be dressed, and to lie on the outside of the bed. Before she was dressed, however, she got an indistinct rigor, and had to be put back into bed.

From this time forward her symptoms underwent a striking change; and she progressively became worse and worse. There was a marked exacerbation of all the febrile symptoms; with hurried respiration, and occasional vomiting. In addition to these it was noticed, that the superficial veins, beneath the clavicles, and on the forepart of the chest, had manifestly become varicose.

From a careful review of her history, and an attentive consideration of her present symptoms, there seemed little room to doubt the existence of inflammation of some of the deep veins of the neck; and though not aware that such a diagnosis had ever been made, still we conceived that no other lesion could satisfactorily explain the peculiar features of her case.

On March 28th she is reported to have slept tolerably well; pulse 120; a deep flush on each cheek; the rest of the face is puffy, and of a chlorotic hue; the belly is soft and relaxed, and everywhere free from tenderness; the network of enlarged veins is still more apparent than heretofore, and includes some branches on the upper arm. The tongue is dry, and has a brown streak down the centre. Her breathing somewhat oppressed; and she continues to have towards evening a short teasing cough; this seems to be her chief source of discomfort; for, unless she moves her head, she experiences no pain in the neck. A slight rigor occurred to-day; in the evening it was remarked that her hearing was somewhat impaired. A rough bruit was detected with the first sound of the heart.

Her condition on the next day was, in every respect, more alarming; the pulse 128, and weak; tongue dry, brown, and crusted; slight subsultus of hands; frequent sickness of stomach; face much swollen; eyes prominent and pupils dilated; deafness increased towards evening;

she complained of indistinctness of vision. Her mind continues clear and undisturbed.

On the 30th it was plain that her dissolution could not be far off. The respirations were frequent and labored; the pulse scarcely countable, and very weak, though the heart was beating strongly; a thick brown crust on the tongue; pupils very much dilated. She was drowsy, and towards evening lapsed into a comatose state, which ended in death at 9 P.M.

Autopsy, twelve hours after death.—The enlarged veins in the neck and chest still very distinct, though changed in color. The abdomen, when laid open, presented no morbid appearance; the liver was healthy. The *uterus* was rather large for this period—twenty-two days—after delivery; its structure was remarkably soft and friable. Behind this organ, and deep in the pelvis, existed a small abscess. The mucous membrane of the large intestines seemed thickened, and was of a very dark color, apparently from intense congestion. The *kidneys* were in an advanced stage of fatty degeneration. On dissecting back the integuments of the neck, and exposing the great venous trunks in this region, it was at once apparent that phlebitis, in its most marked form, had existed here. The deep jugulars, both subclavians, the upper part of each axillary, the right vena innominata, and superior portion of left, were the vessels engaged. In calibre they seemed enlarged, their coats were thickened, and internally they contained firm plugs of coagula and lymph, the latter being the more external, and adhering very closely to the walls of the vessel. These formations extended down to near the superior cava, the lining membrane of which was redder than natural. The pulmonary valves were intensely red, as was the interior of the pulmonary artery,—contrasting strongly with the aorta, which presented its natural color. In the right auricular appendix was a small incipient abscess. The tricuspid and mitral valves were intensely injected, so as to present a bright scarlet color. The aortic valves contained some calcareous matter, but otherwise presented no abnormal appearance. The lining membrane of the heart itself was pale, and showed no traces of inflammation.*

Such is a very brief history of the symptoms and morbid appearances which this case presented. It will be remarked that, at an early period after parturition, the patient became affected with symptoms of low puerperal fever, the gastro-intestinal mucous membrane being the part on which the action of the poison seemed to be chiefly expended, though it is possible this diarrhœa may have had some connexion with the renal disease. A variety of circumstances concurred in this case to favor the development of puerperal fever. The kidneys were far advanced in fatty degeneration; she had spurious pains for a considerable time before the setting-in of true parturient action; her labor was tedious (twenty-seven hours), and was followed by severe hemorrhage. Another circumstance there was in her case, yet more influential than

* The preparation showing these morbid alterations was exhibited to the meeting, and is now in the Museum of the Lying-in-Hospital.

all these together, in predisposing to the invasion of puerperal fever,—she had been seduced, and was laboring under intense mental depression, from the conjoint influence of bitter disappointment, and the cheerless prospect of a life of irretrievable disgrace and shame.

The attack of puerperal fever seemed partially to yield to treatment; there was some mitigation in the local and general symptoms. In the middle of the second week, however, the first indication of phlebitis of the neck showed itself. Now, in most cases of crural phlebitis, we find the course of events to be much the same as that just described. Thus, of sixty cases of puerperal phlegmasia dolens, collected by Dr. Mackenzie, the attack followed upon some form of puerperal fever, in thirty-three instances, and even this proportion I believe to be much under the mark.

The local effects of phlebitis were present, for some days, without being attended with any remarkable constitutional symptoms; and I freely confess that no suspicion crossed my mind, at this period, of the real nature of this swelling in the neck. To this localized tumefaction œdema succeeded; then came a rigor, and after it a sudden explosion of alarming symptoms, which too plainly revealed that the very fountain of life itself was poisoned. At a later period the dilated condition of the superficial veins on the front of the chest attracted our attention, and first suggested the possible existence of phlebitis of the deep veins of the neck. But little reflection was required for this conjecture to settle down into absolute conviction; in fact, for the establishment of this diagnosis, no symptom was now wanting: there were local swelling and tenderness, œdema, unequivocal signs of venous obstruction, and the constitutional disturbance ordinarily attendant upon phlebitis. Mark, also, the sequence in which the symptoms appeared:—First, puerperal fever, and apparently partial recovery; next, localized pain and tumefaction; then œdema; and, some days later, a rigor, with increased constitutional disturbance; and, lastly, varicose enlargement of the superficial veins.

The condition in which the deep jugular veins were found, after death, plainly showed that they had become wholly impervious, so that the return of blood from the head must have been entirely effected through the vertebral and superficial jugular veins. That great obstruction existed in the venous circulation, was manifest during the last few days of her life, and caused the aspect and expression of her face to bear a very close resemblance to those cases in which heart disease, and enlargement of the thyroid-gland, co-exist. I regret exceedingly that an examination of the head could not be obtained.

The subclavian vein on each side was more or less obstructed by firm coagula and lymph; yet there was no œdema of either hand or either arm. It is, perhaps, impossible to say, with certainty, in what particular spot the phlebitis began, or in what direction it spread; that is, whether in a direction towards or from the heart. My own opinion is, that it extended along the veins *contrary* to the course of the circulation, as is generally observed in crural phlebitis. This opinion is founded on observation of the situation of the tumor during life, and of the

morbid appearances in the veins,—the apparently more recent inflammatory deposits being at the remote point from the heart.

The existence of fatty degeneration of the kidneys is a feature in this case that should not be overlooked, particularly at the present time, when so much attention is being directed to the influence which this organic lesion exercises over the progress and results of intercurrent diseases. The urine was not at any time tested, as, owing to the constant presence of diarrhœa, it was not procured free from the admixture of fæcal matter.

I believe it will not be an exaggeration to assert, that the case just detailed, in so far as relates to the phlebitis of the venæ innominata, is almost unique in medical literature. I know of but one similar instance; it was exhibited at the Pathological Society, in November, 1851, by Dr. Mayne, and the account of it is published in the thirteenth volume of the Dublin Quarterly Journal, N. S.

From its interest and great practical importance, I feel that no apology is necessary for quoting Dr. Mayne's account of this case, as submitted to the Pathological Society:—"The case was that of a man aged 29, who had been in hospital in February last, laboring under symptoms of incipient phthisis. After a few weeks he left the hospital, and returned to his business; he was very soon, however, seized with a severe bowel complaint, supposed by himself to be dysentery, and which persisted up to last October, when he again placed himself under Dr. Mayne's care, in consequence of the sudden supervention of pain and swelling in the *right* upper extremity; the pain, commencing behind the clavicle and in the corresponding shoulder, was soon followed by œdematous tumefaction of the entire limb, which was hot, painful to the touch, and pitted imperfectly upon pressure. The deep-seated veins could be traced, hard and cord-like, under the integuments, and the superficial veins about the shoulder, the axilla, and the arm, were largely dilated, and by their blue color and varicose appearance at once attracted the attention of the observer. For the few days during which the man survived, the bowel irritation seemed to supersede the chest symptoms. The stools, which were as many as twelve to twenty in the twenty-four hours, were of an ochery character, and insupportably offensive.

"At the post-mortem examination both lungs were found filled with tubercles, but there was no cavity of any extent in either. All the veins of the right upper extremity were filled with firm coagula, which adhered to the lining membrane, and retained a considerable portion of the coloring matter of the blood. These coagula, when removed from the veins, resembled portions of coral. The coats of the veins were thickened and opaque. These diseased appearances extended as far as the junction of the two venæ innominatæ; the right vena innominata was quite impervious, while the left, the superior cava, and the azygos, were free from any trace of inflammation. The examination of the small intestines was accidentally omitted, but the large intestines exhibited the appearances usually observed in the advanced stage of chronic dysentery; the mucous membrane was covered with ulcers,

many of which in the rectum and sigmoid flexure of the colon were circular, with depressed centres and indurated margins."

Whilst this case resembles, in some respects, the one I have above related, still, between the two there are several points of difference, but which it is needless here to enter upon.

In neither of them was the phlebitis a primary, idiopathic disease. In Dr. Mayne's case it was preceded by phthisis and chronic dysentery; in my case, by puerperal fever and kidney disease.

Respecting the etiology of phlebitis our knowledge is as yet very imperfect. That it may arise from the wound or injury of a vein, is clearly established; though atmospheric condition, and the health of the individual at the time, exercise a powerful influence in modifying the results of this injury. Secondly, phlebitis may result from the immediate contact of purulent or septic matter with the endangium of a vein. That this is not by any means a necessary consequence, however, is abundantly proved by the experiments of Gaspard, Cruveilheir, and Mr. Henry Lee; on the contrary, the blood may be contaminated, and pyemia induced, without any inflammation of the venous canals through which the poison gained an entrance into the circulation. Thirdly, there is good reason to suppose that phlebitis, under certain circumstances, may originate in an extension of inflammation from surrounding tissues. Veins imbedded in bone would appear more disposed than others thus to become implicated in the inflammation of contiguous structures.

Lastly, we sometimes see inflammation affecting venous trunks, independently of any of the preceding causes, and where its production may, in a limited sense, be considered spontaneous; whether or how far it is truly idiopathic, is another and more difficult question to determine. I am strongly inclined to the opinion that phlebitis, thus arising, is not essentially idiopathic; that it is only a consequence, a local manifestation, of toxæmia, or vitiated blood. The circumstances under which this so-called spontaneous form of phlebitis presents itself, lend considerable support to the above view. It is never seen occurring, for instance, in the midst of sound health, but attends or follows upon various diseases whose decided tendency is to alter and deteriorate the blood. In this category we have typhoid, adynamic, and puerperal fevers; phthisical, gouty, and rheumatic states of the constitution; chronic dysentery and chlorosis. Dr. Robert Lee maintains that phlegmasia dolens (or crural phlebitis) is always an extension of inflammation from the venous textures of the uterus. This, be it remembered, is the opinion of one of the highest authorities upon this subject. In it we see a recognition of the fact, of crural phlebitis being *preceded* by a disease eminently favoring the production of blood poisoning. Whether this extension of morbid action does or does not take place, we need not now stop to argue, as the general question is thereby affected in but a small degree.

Dr. Mackenzie's researches on the nature and pathology of phlegmasia dolens have, I think, a close relation to the point now under consideration. He does not deny that phlebitis is always present in the

affected limb. This, indeed, is a fact as well established as any other in pathology; but what he contends for is, that this inflammation of the veins is secondary to, or arises out of, a vitiation of the circulating fluid. In confirmation of this he adduces the results of many direct experiments, and cites a large number of cases of phlegmasia dolens, from various authors, all concurring to give strong support to his pathological views.

I am not going to pursue this inquiry further at present, as it would open up too wide a field for research, and one, moreover, which, to a certain degree, has already been explored by Dr. Mackenzie. In the case already related, and which has led to these remarks, there can be no possible doubt that when the phlebitis of the neck appeared, a state of pyemia, or at least of blood-poisoning, had for some days existed. Her symptoms then, and previously, together with the post-mortem appearances, concur in supporting this view.

In Dr. Mayne's patient the health was broken down and seriously impaired by chronic dysentery and phthisis, for a considerable time previously to the occurrence of the phlebitis; so that we see, in both these remarkable instances, causes were in operation quite adequate to produce an altered, vitiated state of the blood.

It has been stated, in the course of the clinical history of this interesting case, that the local effects of phlebitis were present for some days without being attended with any remarkable or peculiar constitutional symptoms. This leads me to observe here, that of *pure phlebitis we have, I believe, in point of fact, no pathognomonic symptoms beyond the local ones; the symptoms considered to be such belonging rather to the state of pyemia.* Hence, in all cases where a vein or veins, beyond the reach of tactile examination, are inflamed, as, for example, in a case of uterine phlebitis, we cannot know of a certainty that the venous tissue is the one particularly engaged, unless the characteristic symptoms of pyemia show themselves. To this conclusion, at least, I am led by my own experience. I may, perhaps, be in error on the point, and therefore would not wish to speak dogmatically. At all events it is just one of those questions which can only be decided by the collective experience of many observers. It, therefore, with the other question—"Does phlebitis ever occur as a purely idiopathis disease?"—I would respecfully throw out, as subjects on which it would be of advantage to elicit the opinions of others.—*Dub. Quarterly Journ.*

Extract from an Article, entitled Observations on Pericarditis. By Robert Law, M. D., Professor of the Institutes of Medicine in the School of Physic in Ireland.

Have we any sign or signs, upon which we can rely as indicating adhesion of the pericardium?

Dr. Hope remarks: "I certainly consider this diagnosis to be one of the very few connected with the heart which cannot be made with absolute certainty, and I never, therefore, venture to assert respecting it."

He further remarks, that by a combination of signs he has succeeded in detecting it:—First, by the heart's beating as high up as natural in the chest, and causing a prominence of the cartilages of the left præcordial ribs. Second, and which he regards as the most characteristic of all, an abrupt jogging or tumbling motion of the heart, very perceptible in the præcordial region with the cylinder. Third, a history of previous pericarditis, especially if connected with acute rheumatism, affords strong presumptive evidence corroborating the preceding signs, and the absence of such history should make the auscultator pause before he ventures on a diagnosis in stronger terms than that it is probable or possible. With regard to the first sign stated by Dr. Hope, I would say, that I have never observed this high action of the heart and prominency of the cartilages of the left præcordial ribs in adhesion of the pericardium, but I have observed them in another morbid condition of the pericardium, in case of effusion into its cavity, when the fluid, gravitating toward the most dependent part, pushed up the organ towards its base, and produced a prominency corresponding to the base, the appearance which Louis designates "voussure." It is the pushed-up heart that forms the prominency, not the fluid, as is generelly believed.

As to the second sign proposed by Dr. Hope, I can only say, that his description of an abrupt, jogging, tumbling motion, does not describe the irregular action that I have sometimes observed in cases of adherent pericardium. Dr. Walshe's description of it as a tumultuous, confined action goes nearer to conveying an idea of what I have heard. As I have observed it, it has been a kind of pulling, dragging motion, as if one contraction of the ventricle was resolved into a series of short, abrupt contractions, sometimes so feebly and faintly expressed as if no impulse were communicated to the blood by the heart as it passed through it. The jogging, tumbling motion that I have observed has been in quite a different affection, viz., in the weak, gouty heart. M. Bouillaud remarks: "Je ne connois encore aucun signe qui puisse faire reconnaitre les adherences du pericarde." Dr. Sanders thought he had discovered a positive sign of an adherent pericardium in the retraction or dimple taking place during the systole of the ventricle in the epigastrium immediately below the false ribs, and which he ascribed to the diaphragm being drawn in by the ascending motion of the heart. Dr. Hope observes on Dr. Sanders' sign:—"I have searched for this attentively in several cases of adhesion, but have not been able to detect it in any degree that could constitute a sign. Dr. Heim, of Berlin, also proposed this sign. Laennec, speaking of it, says:—"J'ai cherchè inutilement depuis deux ans verifier cette observation chez tous les malades qui presentaient quelque signe de trouble de la circulation, et je n'ai jamais pu aperçevoir le creux dont il s'agit, et dans le nombre de ces sujets il s'en est trouvè plusieurs dont le cœur adherait au pericarde." Dr. Stokes is the last authority to which I shall refer, as being the latest, and who had, therefore, the opportunity of testing the value of the signs of those who preceded him. He remarks:—"I more than doubt that there is any certain physical sign of adhesion of the pericardium, and have never been able to verify the sign of Dr. Hope

of the double jogging impulse." I am bound to say, that none of the signs hitherto proposed has done more than to enable us to assert the likelihood of an adherent pericardium.

The sign which I propose, and with confidence, has this advantage, that we need not have followed the disease in its progress, nor do we require to have had any previous knowledge of the case. I have again and again tested its truth by post-mortem examination, as have others to whom I have communicated it, and have found that it may be relied upon. The sign to which I allude is, "*the persistence of the same extent of dulness to percussion in the præcordial region, no matter what position the individual may assume.*" The area of dulness on percussion in the præcordial region will be the same under every varying position of the body. The heart becomes so braced up that it cannot move as it does in its normal state, when, if examination be made, the patient either lying, or sitting, or standing, the results of percussion will vary accordingly, the dulness being greater in the first position, and less in the two latter. The individual himself, also, is quite conscious of the existence of some solid resisting body within his chest, which does not move in the changes of posture of his body, but impedes its motions.

I have proved this sign in cases where I have seen the patients all through their attack of pericarditis, and also in cases where the adhesion had been already formed, and have never found it to disappoint me. I, therefore, claim for it that it is *the physical sign* that may be relied on in proof of an adherent pericardium.—*Dublin Quarterly Journal of Medical Science.*

Case of Poisoning by Strychnine, with Experiments on Poisoning by that Substance, and Remarks on some parts of the Medical Evidence given on Palmer's Trial. By J. A. LAWRIE, M. D., and J. B. COWAN, M. D.

(Read before the Medico-Chirurgical Scciety, 10th June, 1856.)

On the evening of the 11th of June, 1853, a medical gentleman, aged 22, while laboring under considerable excitement, the result of a debauch, called at a druggist's shop in Glasgow, wrote the following recipe, and obtained the mixture:—℞ *Strychniæ pur.* gr. iij., *spirit vin. rect.* ℥ss., *acid sulph. dil.* M. xxx., *aquæ puræ ad* ℥viij., *misce et solve.* He proceeded to the hotel where he resided, retired to his bedroom, undressed, swallowed the whole of the mixture, concealed the empty bottle behind the grate, and went to bed. He slept, according to his own account, but not very soundly, for about an hour and a half, his rest being interrupted by dreams, some of which were of the most pleasing and delightful description. He awoke in a spasm, uttering loud cries which alarmed the household. On the cessation of the spasm he fainted, and, on coming to himself, requested the servant to go immediately for Dr. Montgomery. On that gentleman's arrival, suspecting that a poison had been taken, he immediately dissolved some sulphate of zinc, with which he had provided himself, in water; but on commencing to administer it, the first drop that

touched the patient's tongue induced a violent spasm, accompanied with loud shrieks and complete opisthotonos. On the subsidence of this spasm, by introducing his finger to the back of the mouth, and carrying the spout of the drinking cup over it, Dr. Montgomery was enabled to get the emetic partially swallowed. He repeated the dose three or four times in succession. Free vomiting having been induced, the inhalation of chloroform was immediately commenced. At 4 A. M., Dr. Lawrie saw the patient, and, in addition to the continuance of the chloroform, administered a stimulant enema. Between the hours of 4 A. M. and 6.30 A. M., nine spasmodic attacks, more or less severe, occurred. The last of these, which seemed to be induced by the application of a cup to the lips, was very intense and prolonged. The patient started suddenly up in bed, his whole frame being in a state of complete rigidity. The respiration, at first impeded, became suspended, and it was only by the long-continued maintenance of artificial respiration that it was restored. The limbs were rigid, and the fingers clenched. The pupils were dilated. During the spasms, evident relief was afforded by forcible extension of the body. In the intervals there were constant twitches of the extremities. The skin was warm and moist. The pulse was at first extremely rapid, but gradually diminished in frequency. The urine was passed with difficulty. The mind was perfectly collected. From 6.30 A. M. till 2 P. M., the patient was kept by Dr. Cowan almost continuously under the influence of chloroform. The twitches remained till the following day, and the patient then rapidly recovered. It may here be noted that the emetics acted well, that vomiting occurred pretty frequently up till 2 P. M. of the 12th, and that shortly after that hour, when it again happened, some of the undigested dinner of the previous day was found among the rejected matters. It may be stated, that the patient had partaken of a most hearty meal at 4 P. M. of the 11th.

In offering some remarks on the case of Dr. Z., and on the experiments which we have made on animals, we shall take the liberty of comparing them with the symptoms observed during life, and the appearances discovered after death in the case of Cook, as given by some of the medical witnesses on Palmer's trial.

1. As regards the history and symptoms, there is in some respects a remarkable similarity between those of Cook and Dr. Z.

(*a.*) The period which elapsed between the ingestion of the medicine, viz., 1½ hours, corresponds almost exactly. In Dr. Z.'s case, we have only his word to fall back upon; but as he had no motive for deceiving us, we think we are safe in assuming that it is as much to be relied on as any similar statement well can be and quite as much as the case of Cook, or any of those quoted on Palmer's trial. Assuming it, therefore, to be correct, it shows that three grains of strychnine may be swallowed in solution on a full stomach; that the patient, a robust youth, may fall asleep and awake about the end of an hour and a half with the characteristic spasms of strychnine poisoning in their most intense form.

In our experiments on animals, the period from the ingestion to the appearance of the symptoms varied from forty-six minutes, the longest,

to seven minutes, the shortest. The following are some of the variations:—

Rabbit,	¼ gr.	first symptom in	30 minutes.
Dog,	¼ gr.	"	31 "
Dog,	½ gr.	"	9 "
Dog,	½ gr.	"	9 "
Dog,	½ gr.	"	8 "
Horse,	15 gr.	"	46 "
Dog,	½ gr.	"	17 "
Dog,	½ gr.	"	7 "
Dog,	½ gr.	"	20 "
Dog,	½ gr.	"	10 "

Taking the dogs getting the same dose, we find the average period for the accession of the symptoms to be little more than eleven minutes.

(*b.*) The scream or screams were well marked in both cases. Dr. Z. screamed repeatedly, and so loudly as to terrify the female inmates of the house, so that they would not enter his room. Animals, especially dogs, poisoned by strychnine make no noise. In not one single instance have they barked, howled, or moaned either before or during the paroxysms. In this respect their demeanor contrasts strongly with that which chloroform induces, the latter substance causing a continuous bark or yelp so long as the power of emitting sound continues.

(*c.*) We cannot compare the period of death in the two cases. But if we assume that Dr. Z. would have died but for Dr. Lawrie's interference, we have seven-and-a-half hours elapsing from the ingestion of the poison, and six from the first appearance of tetanic symptoms to the time, if not of probable death, at all events to that at which the symptoms reached their acme.

In animals these periods were:—

Rabbit,	¼ gr.	death in	22 minutes.
Dog,	¼ gr.	"	66 "
Dog,	½ gr.	"	32 "
Dog,	½ gr.	"	18 "
Dog,	½ gr.	"	16 "
Horse,	15 grs.	"	1 hour 12 "
Horse,	57 grs.	"	13 hours 36 "
Dog,	½ gr.	"	23 "
Dog,	½ gr.	"	13 "
Dog,	½ gr.	"	49 "
Dog,	½ gr.	"	25 "

Selecting the dogs getting the same dose, we have an average period of about twenty-five minutes.

(*d.*) The sensitiveness to touch was exceedingly well marked in Dr. Z., especially about the mouth, and had this sensitiveness deterred his medical attendants (especially Dr. Mongomery in the exhibition of the emetic) from devising means to enable him to swallow, and persevering in their employment, the probability is that he would have died. In

animals, this acute sensitiveness to touch, or rather to *sudden impressions* of any kind, is well marked. A touch or tap acts like an electric shock. It appears to us, however, that it is the sudden first impression or touch, and not the continuous pressure which causes the shock and the renewal of the spasm, a circumstance which appears to be entirely overlooked by the witnesses on Palmer's trial. In Dr. Z.'s case, Dr. Montgomery was able to give the emetic by passing his finger into his patient's mouth, keeping it there, and pouring the liquid over it. Subsequently, we found that the interposition of a wetted fold of handkerchief pressed in contact with the under lip, enabled him to bear the contact of a cup, and to swallow easily. In dogs, if the finger or rod was continuously pressed against any part of the surface, spasms were not renewed. In connection with this point, much stress was laid by the defence in Palmer's trial, on the fact that Cook asked to have his neck rubbed, and that it was rubbed without causing spasms. Now, we have uniformly found that rubbing any part of the surface in dogs does not cause spasm, and that, so far as we could judge, rubbing or scratching their necks was agreeable to them. As death approaches, the sensitiveness to touch diminishes, and its absence is often the first and best indication that the animal is really dead.

(*e.*) The fact that Dr. Z. vomited, we consider important. Some of the witnesses seem to deny the possibility of vomiting when the system is under the influence of strychnine. Dr. Z. vomited about two-and-a-quarter hours after the ingestion of the poison, and about three-fourths of an hour after the first spasm. This we consider an important fact, and well worthy of being borne in mind, on account of its practical curative importance. Animals, so far as our experiments go, never vomit from strychnine alone; but that they may be made to vomit, is shown by the experiments in which chloroform was given.

The diagnosis of strychnine poisoning is of great importance, and one which deservedly was much insisted on in Palmer's trial. The diseases with which it is most likely to be confounded are tetanus, idiopathic and traumatic, epilepsy, and general convulsions.

1. *Tetanus.*—Will the cause assist us? It will certainly assist, but will not be diagnostic. In idiopathic tetanus it will be of little service, and in the traumatic it ought not to be forgotten, that a lesion so slight as to be overlooked both by the patient and his surgeon (as a blister on the foot from a badly fitting shoe*) will excite the disease, and that we have the best authority for stating that it will supervene upon any state of a wound, from the most recent, as four hours after an amputation, to that in which it is either completely cicatrized, or so nearly healed, as to be quite forgotten by the patient. So far as we can discover, this point was very much overlooked in Palmer's trial, and there is no evidence that the surface of his body was examined to ascertain the existence of a recent abrasion, or newly-closed cicatrix.

On this part of the subject, the important question on Palmer's trial

* Dr, Sym, Ayr, in Glasgow Medical Journal.

was, will a syphilitic sore, primary or secondary, cause traumatic tetanus? It appears to us a remarkable fact, that no recorded case or experience in the affirmative could be adduced; but that is no reason for saying that such an occurrence is improbable, far less impossible.* It would not, in the very least, surprise us to meet with tetanus as the result of an acute chancre or ill-conditioned secondary sore, and we would not fear to hazard our professional reputation on the fact that such cases will yet be met with. We are far from presuming to say that the slight lesions observed, or said to have been observed, on Cook's body, were the cause of his fatal convulsions. We speak of the general etiology of tetanus, and we would remind you that in such cases we must not trust to probabilities. Delegation of tumors would seem, *a priori*, to be a very probable cause of tetanus; experience, however, proves it to be one of the rarest. The same remark applies to parturition.

In forming the diagnosis between strychnine poisoning and tetanus from the symptoms, they may conveniently be divided in those which precede the tetanic paroxysm, those presented during the acme of the paroxysm, and those which follow it. On the first there can hardly be any difficulty. Speaking from our own experience, in every instance the symptoms of tetanus began about the neck and jaw, and are always comparatively, and generally *positively*, slow and progressive; while, after strychnine, the premonitory symptoms are slight, the jaw is unaffected, and the paroxysm occurs early, suddenly, and violently.

The most rapid case of traumatic tetanus which our experience affords, occurred in the Glasgow Royal Infirmary in 1847, in which the symptoms set in three or four hours after amputation of the thigh, and death occurred in eight or nine hours after their accession.

In dogs, the premonitory symptoms are—drawing the tail between the hind legs, uneasiness, unsteadiness, and stiffness of the limbs, altogether giving very much the appearance of an animal frightened, except that he does not growl or bark. He then generally falls on his side, in a sudden, violent, tetanic paroxysm. At times he springs from his feet, and falls flat on his side. At others, he tries to run; his limbs refuse to obey him, are pulled spasmodically under him, and he falls forward on his chest and muzzle.

During the paroxysm, the diagnosis, though more difficult, can hardly be said to be obscure. In strychnine, the paroxysm generally follows a state of comparative relaxation; in tetanus, it is merely the aggravation, often not severe or well marked, of a previously-existing continuous spasm; and almost necessarily, the more intense the continuous spasm, the less marked is the exacerbation. The state of the jaw we consider quite diagnostic. In acute tetanus we have never seen it but so firmly closed, that it could not possibly be wrenched open; while in strychnine, the mouth is very often not closed; and if closed, however spasmodically, can always be easily and widely opened. In proof of this, we would adduce the second experiment on the horse, in which, an hour or two before he died, while the paroxysms were on him, and after he had

* Since the above was written, a case has been published in the Medical Times and Gazette of 7th June.

swallowed 47 grains, we found no difficulty in giving 10 more in solution, through a common beer bottle. This in tetanus would be impossible.

The account which we have given of the state of the jaw seems to have been overlooked by the great majority of the medical witnesses. Indeed, Dr. Todd is almost the only one with whose evidence our experience agrees. Dr. Corbett says, that in the case of French, "there was not what is called locked jaw in ordinary tetanus."

The *risus sardonicus*, so much insisted on by Mr. Solly, we cannot look upon as of any real importance, more especially in such a case as Cook's at midnight in November, with one candle in the room, and the face turned from the surgeon.

The effect of sudden impressions, in aggravating or inducing the strychnine paroxysm, is very marked, and has already been adverted to.

In the intervals of the paroxysm, the diagnosis is easy. In tetanus there are slight remissions, but we have never seen intermissions; and, as a general rule, the more rapid and acute the attack, the less marked are the remissions. The mouth can never, in our experience, be opened. In strychnine the intermission is complete. The mouth opens; in dogs, the tongue hangs out, and the breathing is rapid and panting; at times excessively so. In tetanus the tongue is almost always lacerated by the teeth. In strychnine, however sudden and violent the paroxysm, we have never seen it injured.

In epilepsy, tetanic rigidity, properly so called, can hardly be said to occur. The rolling of the head, beating with the arms and hands, foaming at the mouth, and usually complete unconsciousness, not only during, but after the paroxysm, must be quite diagnostic. The state of the mind during the paroxysm must not be too much insisted on, because we believe that the patient is not conscious during the violent strychnine paroxysm (Dr. Z. certainly was unconscious,) and because cases of epilepsy are said to be recorded, in which consciousness was not lost. A careful search, however, has not enabled us to discover any account of these.

General convulsions are easily distinguishable from the spasms of strychnine, by the rapidity of their accession and of their cessation, by the irregularity of the alternate contractions and relaxations of the voluntary muscles, and by the absence of twitches in the interval of the paroxysms.

Post mortem Rigidity.—We need hardly remind this society that the general law is, that the muscular system becomes lax immediately after death, whatever the cause of death may have been, and remains so for a period, varying with the cause of death, the age and previous strength, the season of the year, and many other circumstances. As the body cools, the rigor mortis sets in, and varies in the rapidity of its accession, its intensity and duration, with circumstances and conditions analogous to those above stated. In our experiments the above law has invariably held true. In many of our cases the animal died quite flaccid, and so quietly, that it was not easy to mark the moment at which death occurred; indeed, in one experiment, the body was, by mistake, opened

before life was extinct. In others, death took place during a tetanic paroxysm, and suddenly, but in every instance death was followed by complete flaccidity. As a general rule, the rigor mortis set in early (say in 30 minutes,) but we doubt very much if this is earlier than when death ensues from violence or other unnatural causes. The rigor mortis did not appear to us to be unusually great, or of unusual persistence. Altogether we have not observed any circumstances connected with the state of the muscular system immediately before, at, or after death, on which reliance could be placed as indicating, much less as certainly diagnostic of, the cause of death.

With these convictions we cannot refrain from expressing our surprise at the discrepancy of the medical evidence on this point, and at the great importance which the bench, especially, seemed to attach to it. Nothing can be more loose and uncertain than the opinions of nurses, body dressers, and undertakers, on the comparative amount of rigidity which they allege they have observed, more especially when they speak from memory at the distance of months, and when they have been made aware that their evidence is considered important. It is fearful to think that the innocence or guilt, life or ignominious death of a fellow-man, can in any way be made to depend on such vague uncertainty. But even supposing that all the circumstances connected with the muscular rigidity had been accurately ascertained and correctly sworn to, it appears to us that by far too much importance was attached to it. We doubt very much if, at this moment, any reliable evidence, either recorded or oral, could be adduced in Great Britain, on the difference as to muscular post-mortem rigidity between spasmodic and non-spasmodic disease, much less between any two diseases of the same order. We have seen a good deal of tetanus, and thought we had observed it pretty closely, but if examined as to our personal observation on these points, we should at once confess our ignorance. The truth is, that any observations which are not absolutely accurate are worse than useless, and that the number of medical men who have made this subject one of accurate personal observation is very small.

We were surprised that no allusion was made to the great rigidity which follows cholera; or to the fact, that, in many instances of death from ordinary causes, it is easier to rupture tendons than to bend articulations. Any one who has operated frequently for stone on the dead subject, must be aware that peculiarly contracted states of the hands and fingers is very common, and that the strength of two muscular men is often requisite to bend the knee or hip joints.

That dead bodies are found stiff in the exact position in which death occurred, can be accounted for in two ways:—

1. In the majority of deaths, muscular power has so completely failed before life was extinct, that the laws of gravitation produce no change on the position of the bodies, and, if undisturbed, they stiffen in the precise position in which they died. That this is the general rule, we have no doubt.

2. There is reason to believe that in some rare instances, when death occurs during convulsive spasm, the spasm of life is prolonged into the

rigor mortis without any intermediate relaxation. If we mistake not, one of the most authentic and remarkable examples of this kind occurred in this city some years ago, in the practice of the late Dr. M'Gregor, in which the death-grasp of the dying man refused to relax its hold after death, until the tendons of the fingers were severed by the knife. The death occurred from a sudden attack of epilepsy.

Post-mortem Appearances.—The heart. When examined immediately after death, the right side of heart was generally found contracting very markedly, the left side very slightly. The cavities were invariably full, those of the right side being distended. In animals examined after a lapse of time, the cavities of the right side of heart were always found full of, either completely or partially coagulated blood, the walls of right ventricle being soft, while the left ventricle was less full, and its walls firm and contracted. In one case the left auricle was perfectly empty, but it generally contained a small quantity of blood. In the horse poisoned by 57 grains, the examination was made eleven hours after death. The right side of heart was turgid, both cavities being filled with dark blood, partly coagulated and partly fluid, and traceable into large vessels. The left side was not distended, but the ventricle contained a large fibrinous clot continuing into auricle. In a case, to be afterwards adverted to, in which a grain of strychnia was given at 8.55 A.M., and the administration of chloroform continued till nearly 4 P.M., without the occurrence of a spasm; on its withdrawal, the tetanic spasms manifested themselves, and the dog died. The heart was found enormously distended, and the pericardium contained a large quantity of serous fluid.

The empty state of the heart in Cook's case was considered a very favorable point for Palmer by his counsel and agents. The Crown, however, succeeded in proving that, in two out of the three cases sworn to in court, those of Agnes French and of Mrs. Smith, the viscus was empty. In the third, the Leed's case, the precognition on this point had not been satisfactory, for the Crown counsel, in his examination of Mr. Morley, skilfully avoided all allusion to it. The prisoner's counsel, in cross-examination, elicits the admission that, "the head being opened first, a good deal of blood flowed from the heart, and that, consequently, he was uncertain as to whether the heart was full or empty;" while Mr. Nunneley, a witness for the defence, and who had assisted Mr. Morley at the examination, says—"We had no doubt of the heart being full, but the blood was drained away, owing to the opening of the head, and by the cutting of the large vessels."

With regard to animals, all the medical witnesses examined as to the results of experiments, with a single exception, admit that the heart is always full, especially the right side; those for the prosecution couching the terms of their answer in a milder, those for the defence in a more decided form. The exception is Dr. Todd, who says, "the right side of heart was *not* generally full."

The lungs were found in the horse deeply congested throughout, and the pulmonary vessels contained clots. In four dogs they were also

found more or less congested, while in the remainder they were normal in appearance.

The stomach of the horse was of the usual size. There were a few very prominent rugæ near pylorus, and, as is not an uncommon occurrence, portions of the lining membrane were digested, and a few worms clung to the walls. The stomachs of all the dogs examined were contracted when the viscus was empty, or nearly so; but in two cases, in which it was distended with food, there was no diminution of size. The contraction was very marked near the pyloric orifice, and in this situation, in several instances, there were marks of congestion, and in all peculiar earthworm-like rugæ. These rugæ were more fully developed by the operation of washing. We have since examined the stomachs of dogs not killed by strychnine, and found similar appearances, though not so well marked as in these specimens.

The liver was healthy in the horse, congested in one dog only. In the horse the large intestines were of normal size, while the small bowels were said to be more contracted and firmer than usual. In two of the dogs the latter were decidedly contracted.

The brain and spinal marrow throughout its entire extent were carefully examined in four cases, but did not present the slightest deviation from their ordinary appearances.

After sacrificing what some may possibly consider a more than sufficient amount of animal life, in ascertaining the symptoms during life and appearances after death, we turned our attention to the more pleasing task of discovering an antidote. The beneficial results which followed, or seemed to follow, the inhalation of chloroform in Dr. Z.'s case, naturally led us to try it in the first instance.

Experiment 1.—To a small-sized terrier, ½ gr. of strychnine was given in pill. In eight minutes the tetanic rigidity was complete. Chloroform was given by inhalation, and artificial respiration attempted by pressure on the ribs. In nine minutes the animal died. Results almost precisely similar followed in the second and third experiments of this series.

These experiments showed, that if we waited till the tetanic effect of a poisonous dose was fully established, the respiration was so weak that chloroform could not act. To obviate this, the fourth and fifth experiments were tried.

Experiment 4.—At 8.30, ¼ of a grain in pill was given to a small dog, and the administration of chloroform immediately commenced. In about three minutes it was under its influence, at first struggling considerably, and then barking and howling continuously. A slow regular paddling motion of the limbs then commenced, gradually becoming more rapid. On the animal emerging from the influence of the vapor, this ceased, but immediately began on its re-administration. At 9.36 vomiting occurred. It was left living at 10.15, having exhibited no symptoms of spasm, and shortly after 12 was found dead, the body being slightly rigid.

Experiment 5.—¾ of a grain in pill were given to a dog at 9, when under chloroform. After twenty minutes, the same paddling motion

observed in last experiment began. Chloroform continued till 10.15, no spasm occurring. Found dead at 12, and body quite warm and flaccid.

These experiments having satisfied us of the power of chloroform to retard the action of strychnine, we resolved to prolong its continuous influence almost indefinitely, and with this view instituted the following:—

Experiment 6.—At four minutes to 8 A.M., about half a grain of strychnine in a crumb of bread was given to a young full-sized mongrel terrier, and chloroform immediately administered. It was easily brought under its influence, and then began to bark, and latterly to moan. At a quarter to 10, appearing stupified, very small quantities of the vapor were given. At 11.10 passed urine; at 11.40 recommenced to bark; at 12 passed urine; at 12.50 had an alvine evacuation. Again made water at 1.15; vomited at 3; lay quietly breathing till 3.40. The gentleman who had kindly undertaken to administer the chloroform now thought the danger was over, and therefore discontinued the use of the chloroform, and gave it some bread, which it ate eagerly. At 3.55 had a violent spasm. The spasms continued in rapid succession til 4.30, when the animal died, the body being quite flaccid.

Experiment 7.—One grain in crumb of bread given to a large pointer bitch at 8.55, and chloroform immediately administered. When under its influence, she moaned loudly. The pupils were intensely dilated. Vomiting occurred at 9.45. The animal continued perfectly quiet till 3.45, when she ate some bread and walked about. Eight minutes subsequently she was seized with spasm, and died at 3.55. The body was opened shortly after death, and the heart found enormously distended.

Our mortification at the untoward results of these experiments was very great. As the point to be ascertained was of great interest and importance, the following experiment was begun this morning:—

Experiment 8.—At 8.19 half a grain was given pure to a black terrier, and chloroform inhalation immediately begun. At 9.50 it had a spasm of no great violence, and died.

Experiment 9.—At 8.20 half a grain of strychnine in solution was given to a house terrier. At 4.15 P.M., it was alive, breathing quietly. It lived till 11.35 P.M., being fifteen hours and fifteen minutes.

The result of this experiment, so far as it went, was very satisfactory, proving that chloroform can arrest the influence of the poison. Sulphuric ether was not found on trial to be at all equal to chloroform.

Glasgow Medical Journal.

Letter from Geo. C. Blackman.

BURNET HOUSE, AUG. 4, 1856.

MR. EDITOR:—Since the appearance of the last number of the Lancet, you have assisted me in two operations which for several reasons are worthy of record. I believe with Wiseman, the father of British surgery, that it is the duty of every surgeon to publish his unsuccessful as well as his successful cases. "For my part," he says in his preface, "I have done it faithfully, and thought it no disgrace to let the world see where I failed of success," which failures, he adds, "have attended

all my brethren as well as myself, and will attend thee, also reader, in spite of all thy care and diligence, if thou undertake the employment." The date of this preface was May, 1676 (*vide Chirurgical Treatises,*) and after briefly detailing the cases which have occurred in my practice, I propose to determine to what extent the predictions of the "Parê of England" have been verified, as regards the treatment of *old* dislocations of the shoulder.

About the 10th ult., aided by yourself, I succeeded in reducing by manipulation, without the pulleys, a dislocation into the axilla of 80 days' standing. The reduction was accomplished in a very few minutes, under the influence of chloroform and ether, and the next morning the patient left for the country, in a comfortable condition. Since that I have received no tidings from him. Encouraged by the result in this case, another patient, himself a physician, a tall athletic man, and about 50 years of age, decided to submit to the same manipulation, although his arm had been dislocated for about sixteen weeks. The dislocation was downward and inward, and about the 10th week an unsuccessful attempt, by another surgeon had been made with the pulleys, to which the force of six men was applied for two and a half hours. The patient being under the influence of chloroform and ether, aided by yourself, Drs. Fries, Cary, Graham and Kaufman, I commenced my manipulations, adducing, rotating, abducting, and elevating the arm. These efforts had been made for about ten minutes, and the least possible violence employed, when a tumefaction appeared in the pectoral region, which in a few minutes attained considerable size. Supposing that the axillary artery was ruptured, as no pulse could be felt at the wrist, a ligature was immediately applied to the vessel at the upper part of its course. The operation was performed about 10 o'clock, A. M., and compression of the pectoral region, made by means of a sponge and broad roller. On removing this the next morning, the tumefaction had nearly disappeared. The patient continued comfortable, and about nine days after the application of the ligature, I was compelled to leave the city on a professional visit to Indiana. I left on Friday afternoon and returned on Monday morning, at which time I learned that my patient had died on Sunday morning from hemorrhage at the seat of ligature. Two physicians, his most intimate friends, lodged in the same house with him, but before they reached his bedside the quantity of blood lost was so great that he sank exhausted in about two hours from the first and *only* attack of hemorrhage. Previous to my departure, for Indiana, I had suggested to the physicians in charge, the importance of having compressed sponge at hand, to be used in any emergency of the kind, but this was not used by the attendant; instead of applying pressure instantaneously, he went in search of the physicians, who at that early hour in the morning, were in bed. The time thus lost, unquestionably led to the fatal catastrophe.

I might refer you to numerous instances of success in the reduction of old dislocations—from two to six months standing—which have occurred since the days of Wiseman, but I propose to notice only the accidents by which some of these attempts have occasionally been followed. One of the earliest recorded, so far as we have been able to

learn, is the case reported by Desault in the *Journal de Chirury.*, t. 4, p. 301.

During the efforts of this surgeon to reduce an old dislocation, suddenly a considerable "*tumeur aerienne*" appeared below the clavicle, which Desault attributed to the "*degagemen de l'air amasse entre les cellules rompues du tissu cellulaire!*" In a few days this tumor entirely subsided under the influence of "*astringens et une compression methodique.*" Whether it was the result of a disengagement of air from the lacerated cells of the cellular membrane, as supposed by Desault, or of a rupture of blood vessels, we leave the reader to determine.

It is somewhat singular that Desault should have met with two cases of this extraordinary phenomenon. Pelletan's explanation, in our opinion, throws some light on this subject. In an attempt to reduce a luxation of four months standing, the same kind of "*tumeur aerienne*" appeared. It was opened, and the hemorrhage from the torn artery was fatal (*Clin. Chirurg.*, t. ii., p. 95.)

Malgaigne states that he is acquainted but with a single instance of an "emphyseme veritable" following a reduction, and that is the one reported by Flaubert, in his *Men. sur. plus cas de luxations dans lesquels les efforts pour la reduction ont ete suivis d'accidents graves;* which appeared in the *Repertoire d'anat. et de phys.*, 1827. The patient, a female, æt. 70, screamed violently during the operation, and Malagigne is disposed to believe that the emphysema was independent of the luxation, or the reduction, (*Traite des Tract. et des Luxations*, tom. ii., p. 147.)

Malgaigne, himself, attempted reduction in a case of sixty-eight days' standing, but was forced to discontinue his efforts in consequence of the sudden appearance of a tumefaction in the axilla, and on the shoulder. Ice was applied, and in the course of a few hours the swelling was arrested, and by the twenty-second day, the blood, which he thinks came from ruptured muscnlar branches, was completely absorbed, (*op. cit.* page 150.)

A case occurred to Flaubert, in which, besides the tumefaction, the pulse could not be felt at the wrist. The hand was cold, insensible and immovable. The next day, however, the pulse returned to the wrist, and in the course of twenty-six days the effused blood was absorbed. Froriep lost a patient from a rupture of the axillary vein, which proved fatal in an hour and a half after the operation, (op. cit. p. 151.) The reader may find in the comprehensive treatise of Malgaigne, details of cases in which the axillary artery was ruptured. We pass over those observed by Verduc, Petit, Platmer, Delpech, and that referred to by Sir Charles Bell, in his Operative Surgery. The late Dr. John C. Warren tied the subclavian to arrest the progress of an enormous aneurismal tumor in the axilla, the result of the reduction of a recent dislocation, and of supposed pressure of the operator's boot. In this instance the coats of the artery were so contused that sloughing took place during a fit of coughing, five days after the accident—(*Amer. Jour. Med. Sciences*, vol. xi, N. S., 1846.) In 1824, M. Loudet lost a patient at the hospital in Rouen. The dislocation was of only eleven

days standing, and was complicated with a fracture of the margin of the glenoid cavity, as in the two fatal cases which occurred in the practice of Prof. Gibson of Philadelphia. The latter cases are too familiar to every surgical student to require particular mention in this place. Prof. Gibson, in connection with the report of the above cases, gives briefly the details of a fatal operation by David, of Rouen. The luxation had existed several months, and great force was employed in the reduction. This resulted in an inflammation, mortification and death. Some years since, Lisfranc attempted the reduction in a case of four months standing. He succeeded; but on visiting the patient an hour afterwards he was found dead. His death was attributed to cerebral congestion, as the autopsy showed the axillary artery, veins and nerves uninjured—Vid. *Bul. de la Soc. de Chirurg. de Paris*, Tom. Prem., p. 718.) In the same volume MM. Lenoir, and Larrey, refer to cases in which they had met with lesion of the brachial plexus, giving rise to paralysis, and yet these were recent cases, and the reduction was most readily accomplished. But I will not multiply cases of this kind; those already related will suffice, in the minds of many, to answer the question—At what period of time after a dislocation of the shoulder, is an attempt at reduction justifiable? When Prof. Gibson lost his first patient, he wrote that "should a case, similar in external appearance to that of James Scofield again occur, I shall feel justified in adopting a similar course."—(*Gibson's Surgery*, vol. 1, p. 824, 5th ed.) When he had lost his second patient (John Langton,) he expressed his views as follows, "The conclusions which I am now prepared to draw, are directly the reverse of what I have stated in some of the foregoing pages; I am now disposed to condemn, in the most unqualified terms, all attempts at the restoration of *ancient* luxations of the humerus and other bones—except in cases where the patient is remarkably thin and debilitated, and where there has been little or no inflmamation at the time or subsequent to the displacement."—(*Op. Cit.* p. 351.) At a meeting of the *Societe de Chirurgie* of Paris, July 3, 1850, M. Maisonneuve reported a case in which, after M. Velpeau had failed, he succeeded in reducing a luxation of the shoulder of 12 weeks standing, and elated with this triumph over the veteran of La Charite, he asserts there are but few cases in which, with the aid of chloroform, we may not succeed. "*Quelles resistances y a-t-il a vaincre ici, en effet?*" He asks, "*Il n'y a presque pas d'engrenage; les muscles sont neutralises par le chloroforme; il ne reste donc que des adherences fibreuses: l'on pourra presque toujours les surnonter les rompre.*"—(*Bul de Soc. de Chir.* t. i. p. 716.) But these fibrous adhesions are not the only obstacles to overcome; where the tissues surrounding the head of the bone have become consolidated by inflammation, the axillary vessels and nerves must be in danger of laceration. Perhaps, however, as M. Maisonneuve suggests, this accident may be avoided by "*extensions preparatoires*," as in the attempts to restore contracted limbs to their natural shape.

GEO. C. BLACKMAN.

Western Lancet

Abstract of Meteorological Observations for August and for the Summer of 1856, made in the Twenty-fourth Ward, Philadelphia. By Prof. James A. Kirkpatrick.

1856. August.	Barometer. Reduced to 32°.		Thermometer.			Solar Radiation, 2 P.M.	Rel. Humid. 2 P.M.	Force of Vapor 2 P.M.	Dew Point 2 P.M.	Rain	Prevailing Winds.	Remarks.
	Daily Mean	Mean Daily Range.	Daily Mean	Mean Daily Range	Extreme Range							
	Inches.	Inches.	Deg.	Deg.	Deg.	Deg.	Hunds.	Inches.	Deg.	Inches.	Points.	
1	29.660	.098	80.0	1.0	31	36	47	.638	66.0		(Var.)	
2	29.623	.045	81.7	1.7	27	46	50	.677	67.7		E.	Morning and afternoon cloudy; evening clear.
3	29.534	.089	81.7	1.0	24	44	56	.764	71.2		(Var.)	Cloudy. *Thermometer highest* 97°.
4	29.521	.013	79.0	2.7	18	9	59	.740	70.3	0.104	ESE.	Cloudy, a few drops of rain; thunder at 5, P.M.
5	29.520	.025	73.0	6.7	5		77	.666	67.2	1.775	(Var.)	Cloudy; *rain* from 5½ to 9, A.M.
6	29.577	.056	74.3	6.0	19	42	41	.437	55.2	0.160	NW.	Cloudy; *rain* morning and afternoon.
7	29.567	.022	76.8	2.2	25	10	44	.531	60.7		(Var.)	M. and aft. clear; ev. cloudy, with *rain, thunder and lightning.*
8	29.508	.059	75.2	5.3	16⅛	5½	65	.723	69.6	1.692	(Var.)	Cloudy.
9	29.590	.082	71.3	6.5	19	25	53	.491	58.5		W.	Cloudy; morning light showers; 4¼ until 7, P.M. *heavy rain,* with *hail* for a few minutes.
10	29.623	.032	74.3	3.0	21½	34	42	.460	56.7		SW.	Clear.
11	29.531	.091	78.7	4.3	21	25	59	.711	69.1	0.015	SSW.	Morning and evening clear; afternoon cloudy.
12	29.505	.026	71.8	6.8	15	13	69	.638	66.0	0·072	SW.	Cloudy; morning *rain.*
13	29.478	.060	74.7	5.2	28	34	56	.610	64.7		WSW.	Cloudy; 1½ to 5 P.M. *rain showers.*
14	29.490	.023	75.0	1.7	20	34	47	.544	61.4		(Var.)	Cloudy; evening, distant thunder and lightning.
15	29.587	.097	71.7	3.3	20	36	43	.429	54.7		(Var.)	Morning and aft. cloudy; evening clear.
16	29.656	.069	70.3	1.3	21	32	42	.408	53.4		(Var.)	Clear.
17	29.684	.027	68.0	2.3	21	30	45	.401	52.9		NW.	Clear; evening, *lightning* in North.
18	29.656	.028	71.0	3 0	22	17	50	.477	57.7		(Var.)	Clear.
19	29.394	.262	70.7	3.7	10		95	.771	71.5	1.165	(Var.)	Morning and afternoon clear; evening cloudy.
20	29.188	.206	70.3	1.3	10	3½	64	.495	58.7	0.008	(Var.)	Cloudy; *raining all day.*
21	29.294	.105	68.7	2.3	17	12	62	.488	58.4		(Var.)	Morning *rain;* aft. cloudy; ev. clear. *Barometer lowest* 29.156.
22	29.540	.247	68.3	2.3	21	28	47	.395	52.5		NW.	Cloudy, a few drops of rain about noon.
23	29.556	.069	75.7	7.3	29		52	.571	62.8		SW.	Morning cloudy; afternoon and evening clear.
24	29.466	.090	73.0	4.0	11	17	49	.482	58.0		W.	Clear.
25	29.497	.058	64.7	8.3	15	16	69	.475	57.6		NW.	Morning cloudy; afternoon and evening clear.
26	29.761	.264	59.0	5.7	21	36	93	.420	54.2			M. cloudy; 1½, P.M. high wind and a few drops of rain; wind storm from the North, continued about 20 minutes.
27	29.833	.073	65.3	6.3	28	33	56	.483	58.0		W.	Clear; night very heavy dew. *Thermometer lowest* 47°.
28	29.754	.080	70.5	5.2	23	12½	48	.449	56.0		SE.	Clear; morning first *hoar frost* observed. *Bar. highest* 29.846.
29	29.525	.228	72.7	2.2	16	9	70	.689	68.2		SE.	Morning and afternoon cloudy; evening clear.
30	29.632	.107	63.0	9.7	18	14	48	.353	49.4	0.195	SW.	M. and aft. rain showers; between 3 and 4, P.M. *th'r. and l'ng.*
31	29.768	.135	66.3	5.3	28	37	47	.464	56.9		NW.	Morning and evening clear; aft. cloudy.
Means for Aug.	29.565	.092	72.2	4.1	20	24¾	56	.545	60.8	5.186	SW.	Morning and afternoon cloudy; evening clear.
" Summer.		.093	76.5	4.2	53		52	·558	61.4	7.990	S. 89° 15′ W. 25-100.	
											S. 68° 33′ W. 36-100.	

The Monthly Range of the Mercury in the Barometer was 0.690 of an inch, and in the Thermometer 50°.

The Barometer is about 210 feet above ordinary high water in the Delaware River. The exact Latitude and Longitude have not yet been ascertained.